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VOL. 42 NO. 388 APRIL 1956

THE INSTITUTE OF PETROLEUM

AN Ordinary General Meeting of the Institute of Petroleum was held at 26 Portland Place, London, W.1, on 14 December 1955, the Chair being taken by Lt.-Col. S. J. M. Auld, O.B.E., M.C., D.Sc., President of the Institute.

The General Secretary read the minutes of the meeting held on 9 November 1955, which were confirmed and signed as a correct record. He also announced the names of members elected since the previous meeting.

The Chairman introduced G. W. Sears, who then presented the following paper in summary:

STATISTICS AS AN AID TO SPECIFICATION *

By G. W. SEARS † (Associate Fellow)

SUMMARY

Existing specifications for petroleum products have evolved gradually, with the result that some of them place unnecessary restriction on the refineries and yet fail to achieve their primary objective of ensuring satisfactory field performance.

The part that can be played by statistical methods in deriving, evaluating, and utilizing correlations between field performance and inspection test data is enlarged upon, and the utility of test methods in relation to their experimental errors and to manufacturing variability is discussed.

The rationale of designing specifications is examined with a particular view to retaining maximum flexibility in the refinery combined with satisfactory field performance. This leads to the view that customers should continue to place their trust in the integrity of the oil companies rather than attempt to ensure consistency of manufacture by imposing additional controls on a wide variety of inspection features, many of which may have no direct influence on the performance of the product.

The paper concludes by considering whether satisfactory correlations between field performance and inspection data may not in the future form the basis for fully automatic plant and refinery control.

INTRODUCTION

THE petroleum industry rightly enjoys an excellent reputation for the high and consistent quality of its products, and this reputation is safeguarded by a complex system of specifications applying throughout all stages of manufacture, and by the very considerable body of co-operative effort which is continually being devoted to the development and improvement of the test methods upon which these specifications are founded.

The use of statistical techniques in this field is becoming increasingly widespread, to the extent that it would be impossible in a single paper to present a comprehensive and detailed account of the techniques available, many of which are already extensively documented under the heading of Statistical Quality Control.

The objective of this paper has therefore been limited to consideration only of those specifications which form the basis for sale between producer and consumer, and it will be assumed that the primary objective of such specifications is to ensure satisfactory product performances in the field.

Performance specifications in the petroleum industry are the products of a process of evolution. As the balance of practical performance required of a product in the market has changed, so specification limits have been adjusted and new test methods have been developed for the control of new requirements. These new test methods have often been incorporated into existing specifications without particular attention to their interplay with the other features already controlled by those specifications.

It is salutary therefore to review these specifications both individually and in their inter-relation one with another, and the current level of activity in obtaining more reliable information on the precision of standard test methods makes the present time opportune for such a review.

While specific test procedures and specifications have been chosen to illustrate the themes discussed in the paper, it should be understood that these have not been deliberately singled out for scorn or praise, for they could be paralleled by a variety of corresponding examples throughout the industry.

GENERAL NATURE OF SPECIFICATIONS

A typical specification consists of both qualitative and quantitative controls. The former, such as requirements for odour of products, are not considered in this paper; those which are quantitative may require adherence either to maximum or to minimum values of the inspection criteria, or in some cases to both.

It is convenient to illustrate the argument which

* MS received 14 November 1955.

† The Shell Petroleum Co. Ltd.

follows in terms of an existing specification, and Defence Specification DEF/2401 for automotive gasoline M.T.80 is used for this purpose. In this specification there is a maximum limit on Reid Vapour Pressure (R.V.P.) of 8 p.s.i., and both maximum and minimum limits on the 10 per cent evaporation point at 70° C and 60° C, respectively. If the 10 per cent evaporation point and the R.V.P. are taken as the axes of a graph, and the specification limits are drawn in, then a rectangular area open at one end is obtained, within which the point represented by the inspection data for the product is required to fall. This area is represented by the shaded portion in Fig 1.

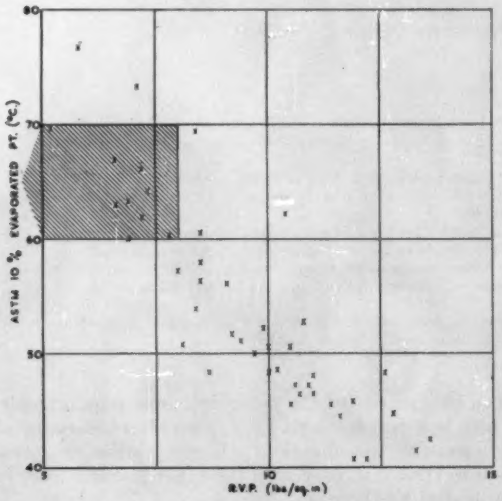

FIG 1

CORRELATION BETWEEN R.V.P. AND ASTM 10 PER CENT EVAPORATED POINT

Among other requirements, DEF/2401 places maximum and minimum limits on the 50 per cent evaporation point, and if this is plotted along a third axis perpendicular to the other two, then the three sets of limits will be seen to form a rectangular box open at one end within which the point represented by the inspection data on the product is required to fall. The space within this box is composed of all those points which represent on-grade product, and all points outside the box represent off-grade product.

Although it is impossible to construct a model involving more than three dimensions, the process of incorporating other limits in new dimensions can be imagined to be extended until all the controls imposed by the specification have been represented. In aggregate they will define a multi-dimensional box some of whose "ends" will usually be open. Some of these open "ends" are, however, closed by implication either by other limits in the specification or by the natural limits imposed on physical grounds. Thus, for example, a final boiling point (F.B.P.) of 205° C max is specified by DEF/2401; although no minimum limit is stated for F.B.P., such a limit is implicit in the requirement that the 90 per cent evaporation point should be 132° C min. Again, although only a maximum value is imposed for R.V.P. it is clear that this cannot be less than zero, and this imposes a natural lower limit.

Analogous considerations may be applied in respect of field performance. It may be assumed that a certain limited number of performance features will be required to be met; for an automotive gasoline these might be anti-knock quality, startability, warm-up, freedom from vapour lock, oxidation stability, and so on. Provided that each of these performance criteria can be measured on a continuous scale, then the requirements of the specification can be plotted directly in terms of the performance criteria, and this would again lead to a multi-dimensional box, some of whose ends will in general be open. It will be convenient to call this the performance feature box so as to distinguish it from the inspection data box.

Now, if the specification limits in terms of inspection data are to succeed in controlling the field performance of the product in all its features at exactly the intended performance levels, then there must be a unique correspondence between the two boxes which have been described. In particular, the boundaries of one box must be equivalent to the boundaries of the other. For example, if it were true to say that the Research Octane Number of a gasoline wholly defined anti-knock performance, then the anti-knock boundary in the performance feature box would correspond exactly with the F1 boundary in the inspection data box. Any point just outside the F1 boundary in the inspection data box must automatically correspond to a point which is outside the anti-knock boundary in the performance feature box.

The number of dimensions necessary to define the performance feature box will be equal to the number of performance features which it is required to control. The number of dimensions of the inspection data box may be either greater or lesser than that of the performance box; if more than one inspection criterion is necessary to control a single performance feature, or if additional inspection criteria are introduced so as to strengthen assurance on consistency of manufacture, then this will result in the inspection box having more dimensions than the performance box. If, on the other hand, some of the inspection criteria serve to control more than one performance feature, then this can lead to a situation where the inspection box has less dimensions than the performance box.

The performance feature box encloses the whole of the space which represents products which will be satisfactory in the field. As such the whole of this space ought to be at the disposal of the manufacturers. The space within the inspection data box on the other hand is the space actually at his disposal. The interrelation between the two boxes will be made clearer by consideration of the nature of correlations between measures of performance and inspection data.

QUALITY AND ITS CORRELATION WITH INSPECTION DATA

One of the difficulties encountered in drawing up specifications in the petroleum industry is that the quality required of petroleum products is often inherently difficult to define because many such products are intended for multi-purpose use rather than for a limited and specific application. Motor gasolines in any one market, for example, are required to be suitable over a wide range of operating conditions, both in respect of the appliances in which they will be used and of the atmospheric and general local conditions.

This point has been discussed by Kendall [1] when he considered the specification of the quality of engine lubricating oils.

Even when it is possible to obtain a clear definition of the performance features which it is required to control, it is seldom easy to measure these performance features in the field let alone to decide which inspection data, either individually or collectively, will exercise control appropriate to these features. To the extent that the product may be required to satisfy an aggregate of appliances under a variety of conditions, however, statistical methods may serve a useful function by defining these aggregates in terms of appropriate mean values and distribution curves, but it is not proposed to dwell on this aspect.

For the purposes of further discussion, it is assumed, somewhat naïvely, that all the performance features which it is required to control can be defined and, if necessary, measured in the field. It is then necessary to consider whether it is feasible to control any or all of these features by imposing limits on suitably chosen inspection data.

The ideal case discussed in the foregoing section, in which each boundary of the inspection box corresponds exactly to one of the boundaries of the performance box, can be realized only if there is a one to one correspondence between the limits imposed and the performance features which they control. Moreover, the series of true correlations between the performance features and their corresponding inspection data must be perfect. A true correlation in this context is to be understood as the relationship between actual or " true " values of performance and inspection data, as opposed to an observed correlation which represents the relationship between experimentally determined values of performance and inspection data. A true correlation may be said to be perfect when all plotted " true " points lie on a smooth curve.

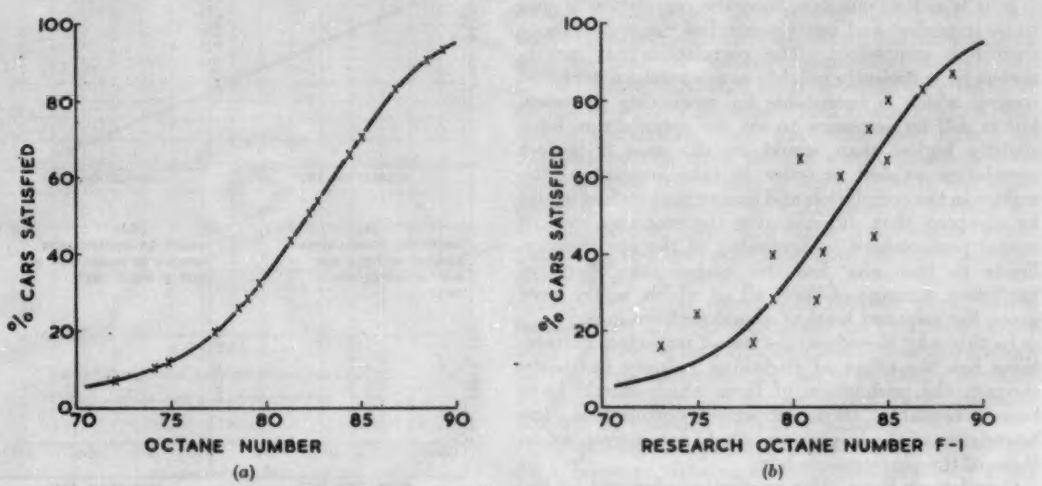

FIG 2

PERFECT AND IMPERFECT CORRELATIONS (DIAGRAMMATICAL ONLY)

(*a*) Perfect correlation; (*b*) imperfect correlation

Note. All plotted points represent " true " values

The anti-knock quality of a motor fuel might, for example, be defined in terms of the percentage of cars which will give knock-free performance on that fuel in a given market and under certain arbitrarily specified running conditions. Then in theory, it would be possible to determine the exact performance of any fuel simply by running all cars in the market on this fuel and observing whether knock is obtained under the specified running conditions. Equally, it would be possible by engine rating the fuel in a very large number of laboratories to determine, say, the Research Octane Rating of the fuel to a very high degree of accuracy. By following this procedure on a wide variety of fuels of all types it would be possible to plot the proportions of cars knock-free against the

corresponding octane ratings, and if the correlation between the two is perfect, then a smooth curve should result as indicated in Fig 2 (*a*).

In fact, no such smooth curve results because the Research Octane Rating is not unique in determining the anti-knock performance of a fuel in practice, and the resultant plot would be of the type illustrated in Fig 2 (*b*) in spite of the fact that all points are absolutely accurate. The data illustrated in Fig 2 (*b*) which are scattered about a general trend are typical of the correlations normally used in practice. The scatter about the trend may itself be related to other inspection data, for example to the Motor Method Ratings in the case under discussion, but it is seldom possible to relate the scatter wholly to any of the other inspection data incorporated in the specification.

If it is not so possible, then the correlation is said to be imperfect and exact control of the performance feature is impossible. The correlation may nevertheless be sufficiently reliable as to exercise a degree of control which is acceptable for marketing purposes, but it will be necessary to set the specification limit slightly higher than would be the case if perfect correlation existed in order to take account of the scatter in the correlation and ensure that no fuel would be accepted that did not give the required level of actual performance. Tightening of the specification limits in this way has the undesirable effect of excluding a range of fuels all of which would have given the required level of actual performance.

In this way therefore, the use of imperfect correlations has the effect of curtailing refinery flexibility through the prohibition of fuels which should have been acceptable. With imperfect correlations, the boundaries of the inspection box do not correspond to those of the performance box.

Correlations in practice are further obscured by the existence of experimental errors both in the measurement of field performance and of the inspection data. It would be quite impracticable to measure anti-knock quality in more than a few hundred cars, and even this could not be repeated as new fuels are developed and feedstocks changed. Equally it is impracticable to measure the inspection data to the degree of accuracy that is necessary to make it immediately clear without analysis whether the correlation is or is not suitable for control purposes.

Statistical techniques can, however, be of considerable assistance in this matter because, provided the data are so collected as to represent a wide variety of fuels, or whatever other products may be involved, and as to provide appropriate information on the precisions of the performance measure and of the inspection data, then it is possible to obtain a direct estimate of the amount of scatter about the true correlation and hence to determine how satisfactory the correlation will be for purposes of specification. A simple example of this technique as applied to the manufactured data of Fig 3 (*d*) is presented as Appendix I to this paper.

The data illustrated in Fig 3 have been deliberately manufactured in order to indicate the effects of experimental errors in the case where the underlying correlation is perfect. Fig 3 (*a*) illustrates the true correlation which has been assumed, and Fig 3 (*b*) represents the same data when experimental errors are present in the measurement of field performance but not in the measurement of inspection data. Fig 3 (*c*), on the other hand, represents the reverse situation when experimental errors are present in the measurement of the inspection data but not in the measurement of field performance. Finally, Fig 3 (*d*) represents

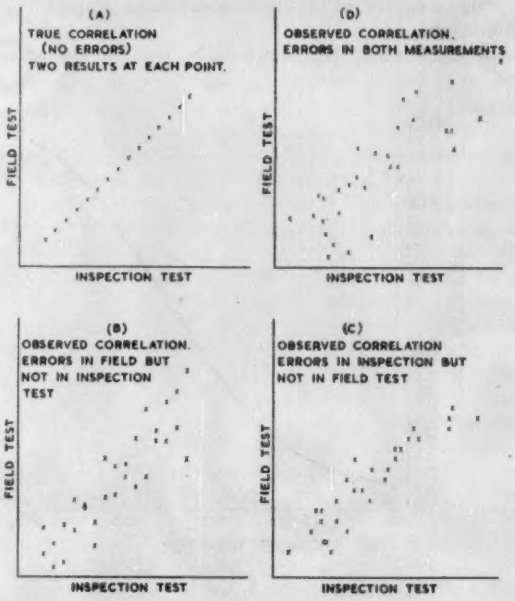

FIG 3

THE EFFECTS OF EXPERIMENTAL ERROR ON PERFECT CORRELATION

the case when experimental errors are present both in the measurement of field performance and of the inspection data.

In manufacturing the data for Fig 3, the experimental errors were chosen at random, but were of fixed precision; *i.e.* a fixed reproducibility was assumed for the inspection test and for the field test. It will be clear that the worse the values of reproducibility assumed, the worse would have been the scatter of the points in the final diagram, where errors are incorporated in both test methods. Equally, had the true correlation assumed been imperfect, in which case scatter would already have been present in Fig 3 (*a*), then again the scatter in the final diagram would have been worse than that illustrated.

Conversely, when an actual set of correlation data is under discussion, so that the observed final scatter is fixed, the worse the precisions of the field and inspection tests are, the better may be the underlying correlation. For, provided the reproducibility of the

inspection test were sufficiently bad, then this alone could account for the whole of the observed scatter so that the true correlation might well be perfect. If, on the other hand, the reproducibilities of the inspection and field tests were both very small, then the vast majority of the observed scatter must be owing to the true correlation's imperfection.

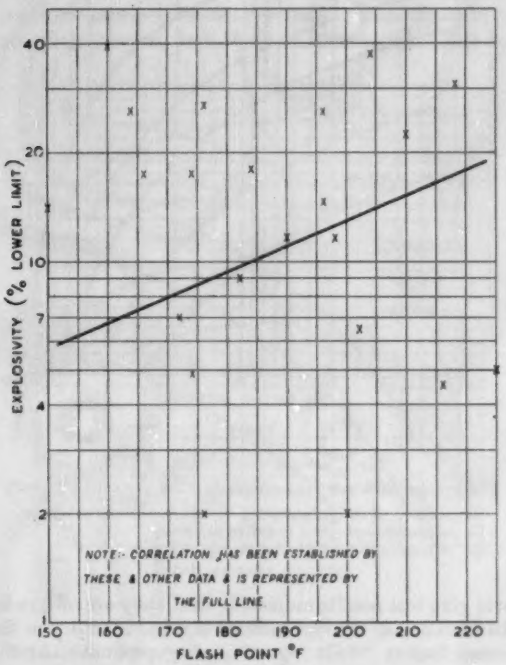

FIG 4

CORRELATION BETWEEN EXPLOSIVITY AND FLASH POINT

It may appear somewhat paradoxical that, given any particular observed set of correlation data between an inspection test and a performance test, the worse the precision of the inspection test, the better may be the underlying true correlation, but this is the case.

As an actual example, the correlation between the Admiralty Explosivity Test and Flash Point may be considered. If it can be assumed that the Admiralty test provides a satisfactory measure of the explosive hazard in the field of fuel oils, then it is pertinent to enquire whether this test itself must be written into the specification or whether suitable control cannot equally be obtained by placing an appropriate limit on the Flash Point. A correlation plot of typical data is presented in Fig 4. While these data appear unpromising in the extreme, it is still possible that a satisfactory underlying correlation exists but is obscured by large experimental errors in one or other, or more probably in both of the series of measurements.

It has, in fact, been established by means of the data presented in Fig 4, together with others not there presented, that a correlation between the two methods does exist, this correlation being represented by the full line in Fig 4, but in this case the experimental errors in the two test methods are not sufficiently large to explain away more than a small proportion of the total scatter. It appears therefore that "explosivity" as measured by the Admiralty test is related not only to Flash Point but to other inspection features as well.

Indeed, this is by far the most common situation. Performance features are usually related to more than one of the inspection data, and this automatically rules out one to one correspondence between the boundaries of the performance box and those of the inspection box. For if only the inspection feature which is dominant in the correlation is incorporated in the specification, then this is equivalent to the use of an imperfect correlation which has already been seen to lead to non-correspondence between the boxes. If, on the other hand, account were taken of all the inspection features which entered into a particular correlation, then the limits that should be imposed on one such feature would themselves be dependent on the quality levels reached in the other features. This would be equivalent to cutting off corners of the inspection data box, which would therefore have less boundaries than the corresponding performance feature box.

Moreover, linear correlations between performance features and inspection data are the exception rather than the rule, so that although it is reasonable enough for the performance feature box to be wholly bounded by plane surfaces, the corresponding inspection data box will be bounded by curved surfaces.

By placing independent limits on the inspection data, however, existing specifications implicitly insist on a wholly rectangular inspection box, and this must result either in the acceptance of fuels outside the required performance range or alternatively, and more probably, in the prohibition of fuels which would have given entirely satisfactory performance. Since the links between inspection data and performance are currently of a tenuous character, it is clear that existing specifications impose quite unnecessary restrictions on the refineries by excluding whole ranges of products which would give satisfactory field performance. This is a result in part of incomplete knowledge of the relationships between inspection data and performance, and in part of the use of inappropriate test procedures.

Whether this situation can be alleviated is a matter for debate. There is little doubt that improved correlations between field performance and inspection data could be developed, at least provided that more than one test method may be used to define any one performance feature. The use of such correlations would, however, require much more complex specifications than those at present in use, and might well be unacceptable to the industry on this score. A more satisfactory remedy, undoubtedly, would be to develop new test methods which possess more direct

relationships with the performance features which it is required to control.

PRECISION OF TEST METHODS

Until quite recently, test methods have been widely used for purposes of specification with, at best, only a hazy notion of their precision. Quite apart from their utility as performance controls, it is pertinent to enquire whether test methods used for specification are sufficiently precise for this purpose. The dominating factor in this respect is not the absolute precision of a method but its precision in relation to the precision of manufacturing, as will be seen from the following consideration of a test procedure exercising a single limit constraint.

Assume that a succession of " batches " of product is to be produced to a given specification, and that the conditions of manufacture remain stable over the period of time involved. A " batch " in this connexion means a single tank lot from which deliveries are to be made; whenever such a tank lot is either topped up or replaced, then a new " batch " is created. Assume also that the process is set so as to produce a majority of batches inside specification, this being achieved by aiming slightly inside the specification limit. For the time being, the economic

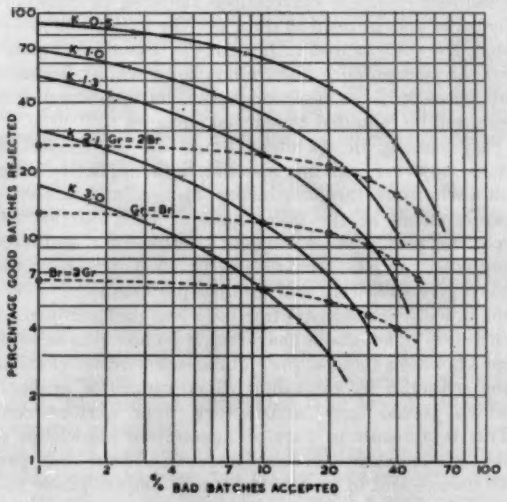

FIG 5

SINGLE-LIMIT CASE

Production 88·5 per cent on-grade; 11·5 per cent off-grade

K = ratio of manufacturing to test method variability
Gr = number of good batches rejected
Br = number of bad batches rejected

consequences of such action will be ignored, but some remarks on this point are made in a later section.

It will be clear in general terms that if the manufacturing process and conditions are reasonably constant and the manufacturer aims sufficiently far inside the specification limit, then the whole of his product will be one grade in fact. Nevertheless, if an imprecise test method is used to check the quality of the product, quite a large proportion of the batches

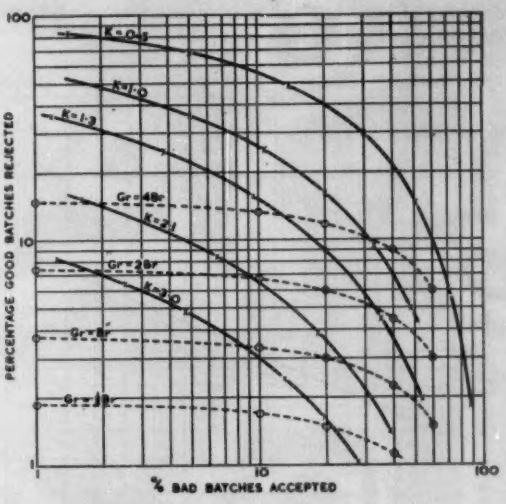

FIG 6

SINGLE-LIMIT CASE

Production 96·4 per cent on-grade; 3·6 per cent off-grade

K = ratio of manufacturing to test method variability
Gr = number of good batches rejected
Br = number of bad batches rejected

will give test results indicating that they are off-grade. In such a case the test method is unsatisfactory in the sense that it " fails " good-quality product. At the other end of the scale, if the manufacturing process is rather variable, but the test method is very precise, then each individual test result will correctly reflect the true quality of the batch; the vast majority of good batches will then be accepted, and the vast majority of bad batches will be rejected. Intermediately, if the precisions of manufacture and of the test method are both rather poor, then unless the manufacturer introduces an inordinate margin inside the specification limit, the process will yield quite a number of off-grade batches, some of which will be passed by the test procedure, and a number of on-grade batches, some of which will be " failed " by the test procedure.

The proportions of good batches so accepted and bad batches so rejected depend on the ratio of the variabilities of the test procedure and of the manufacturing process. Provided a number of assumptions is made, the situation is amenable to quantitative analysis in general terms, and this has been carried out in the manner indicated in Appendix 2. The results so obtained are summarized in Figs 5–7.

In making this analysis it has been assumed that the inspection procedure will consist of obtaining only a single test result on each batch, and of passing the

batch, provided that this test result indicates that the product is on-grade. Where, on the other hand, the test result indicates that the product may be off-grade, the batch is regarded as "failed."

Figs 5, 6, and 7 represent respectively the cases in which the manufacturer sets his process so as to produce approx 11, 4, and 1 per cent of off-grade batches, and it is believed that these cover the usual working range. The individual curves in these figures represent a constant variability ratio between the manufacturing process and the test procedure. Moving

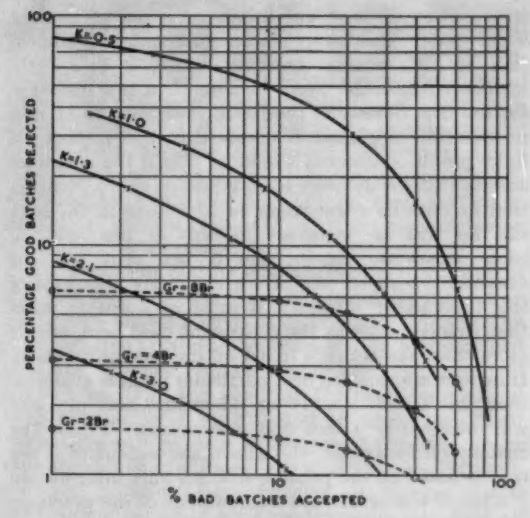

FIG 7

SINGLE-LIMIT CASE

Production 99·2 per cent on-grade; 0·8 per cent off-grade

K = ratio of manufacturing to test method variability
Gr = number of good batches rejected
Br = number of bad batches rejected

along any one of the curves represents changing the limit at which the manufacturer proposes to pass the product on the basis of a single test result. For example, in Fig 5, on the curve for $K = 1{\cdot}0$, which represents equal variability of manufacturing process and test procedure, it will be seen that it is possible to restrict the proportion of bad batches accepted to approx 10 per cent, only by rejecting some 35 per cent of the good batches. The proportion of good batches rejected can be reduced only at the expense of accepting a higher proportion of bad batches. The case quoted here represents the "rejection" of twice as many good batches as bad. For a given value of K the situation becomes increasingly unsatisfactory as the proportion of bad batches made is reduced. Reference to Fig 7, which represents the situation when the proportion of bad batches made is about 1 per cent, shows that when only 10 per cent of the bad batches made is accepted, the corresponding figure of good batches rejected has dropped to about 8 per cent, but at this point the proportion of good to bad batches rejected is nearly 10 : 1 Such a situation is clearly unacceptable to the manufacturer.

The value of K which must be exceeded before a test method can be regarded as satisfactorily fulfilling the purpose of the specification must be largely a matter of opinion. In the author's opinion, a value of at least 2 is required; *i.e.* a test procedure should be at least twice as precise as the manufacturing process.

It would be possible in theory to increase the value of K representing the ratio of the variabilities, by carrying out two or more tests on each batch, but the amount of improvement so obtained is limited not only by the practicability of such an inspection procedure but also by the tendency for test methods used in the petroleum industry to have appreciably better repeatabilities than reproducibilities. It is still not generally appreciated that if the reproducibility of a test method is poor, then good repeatability may be more of a liability than an asset in the sense that repeat determinations do not materially improve the precision of the overall result. That this is so may be seen by consideration of the limiting case in which repeat results agree exactly, *i.e.* in which the repeatability is zero; for in this case all results will be identical, and hence the overall result and its precision are both unaffected by repeating the determination.

It may be asked whether the practice of carrying out repeat determinations on batches which are "failed" on the first test result does not appreciably improve the performance of the test method. The answer to this is unequivocally "No," for, in the first place, the initial test will have already let through its proportion of bad batches, and in the second place, the fact that most test methods have appreciably better repeatability than reproducibility will automatically ensure (rogue results excepted) that good batches which have been "failed" on the first test will almost certainly be "failed" on the repeat. Unfortunately, a quantitative analysis of this aspect is not easily possible in general terms, since it would involve the introduction of two new variables, namely the ratio of the repeatability to the reproducibility, and the freedom to adjust the rejection limit for the mean of the two results to some new level. Even for a specific case, the computations required for such an analysis are rather extensive and are beyond the scope of the present paper.

In regard to the case of a double limit specification, rather similar conditions are found to apply, but in this case it will be assumed that the manufacturer sets his process so as to produce, on the average, a quality which is exactly midway between the specification limits. The parallel between the single and double limit cases is not quite exact, since in the latter a product which is off-grade on the high side can conceivably be "failed" through giving results on the low side, and vice versa. Where the double limits are intended to ensure constancy of manufacture, as is the case, for example, with penetration of bitumen, then the width of the tolerance limits should be decided in relation to manufacturing variability

without reference to the precision of the test method concerned. Poor precision of a test method, in fact, is not considered to justify the production of more variable product than the customer requires. Nevertheless, where the test method variability is large in comparison with the tolerance limits it must be fully appreciated on both sides that a consistent and entirely satisfactory product will quite frequently give off-grade results, and these should only be used with caution in deciding whether or not the product should be rejected or reprocessed.

Analogous calculations to those presented in Figs 5–7 have been made only in the double-limit case when approximately 7 per cent of batches made is off-grade. The results of these calculations are presented in Fig 8, which will be seen to be of essentially the same character as the curves drawn for the single-limit situation.

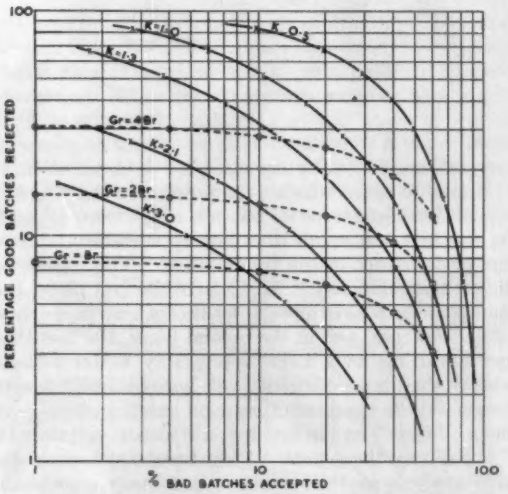

FIG 8

DOUBLE-LIMIT CASE

Production 92·8 per cent on-grade; 7·2 per cent off-grade
K = ratio of manufacturing to test method variability
Gr = number of good batches rejected
Br = number of bad batches rejected

OFF-GRADE RESULTS AND THE CUSTOMER

Statistical derivation of the precision of test methods carries with it the advantage of being able to assign an accurate probability to obtaining a given test result when the true quality of a batch is known. For example, it may be stated that if a fuel of cetane number 55 were tested by a large number of laboratories, then only 5 per cent of the results would lie below:

$$(55 - 0{\cdot}84 \times 4)/\sqrt{2} = 52{\cdot}5 \text{ (approx)}$$

Note: The reproducibility of 4·0 cetane numbers for IP 41/48T has been determined from extensive correlation data obtained under the auspices of the Ignition Quality of Diesel Fuel Panel of the Engine Tests Sub-Committee ST-5. The method of making the above calculation is indicated in Appendix E to Standard Methods.

Predictions such as this will, however, be accurately realized only if a large number of results is involved.

Similarly, as indicated in Appendix E to Standard Methods,[2] it is correct to say that if a customer obtains a single result of 52·5 cetane on a batch of fuel, then the "chance" that this batch does in fact meet a specification of 55 cetane minimum is not more than 5 per cent. This latter statement requires qualification on two counts, however; first, it is strictly incorrect to use the word "chance" in this instance, for the true quality of the batch is already fixed, and no question of chance is involved in this respect; a more precise statement would be that if the customer asserts that his delivery is off-grade on every occasion that he obtains a test result of 52·5, then in the long run he will be incorrect in only 5 per cent of such occasions. Again, the 5 per cent will be accurately realized only if a great many occasions of this type arise; secondly, it must be borne in mind that chances, such as the 5 per cent used here, are no more than assessments based on limited information. If further information on a particular point is available then this should usually be taken into account, and will necessarily affect the assessment. From the customer's viewpoint, the above assessment of 5 per cent is based on the premise that his only information is a single test result and a knowledge of the precision of the test method. A very different assessment would undoubtedly be obtained if he were prepared to accept that no contamination or loss had occurred, or if he were prepared to take into account the test result obtained by the manufacturer before delivery. A more debatable point is whether he should always take into account, in addition, his knowledge gained from previous deliveries of the reliability of the manufacturer concerned.

It is fortunate for the manufacturer that he has an advantage in this respect for he will have access, not only to the final test data on the blends, but also to continuing inspection data and experience on all the blending streams which go to make up the final product, and will thus be able to make a much more reliable assessment of final quality than the customer. In fact, this seems to be a case where the manufacturer will more often be right than the customer!

The whole of the preceding argument has tacitly assumed that the manufacturing and delivery process will not give rise to "gross errors" in quality. The assessment has, in fact, been based on the performance of test methods in segregating bad batches from good in normal manufacture. To the extent that mild contamination or loss in transit will give rise to batches whose quality is not materially different from normal production, the results obtained so far may be considered to be approximately representative of the

discrimination which will be achieved by test methods in such circumstances. Heavy contamination or loss, or a gross error in manufacture, such as delivery of the wrong grade, are very rare events in the petroleum industry, and are in any event easily picked up even by the less sensitive test methods, and as such, will not be considered further in this paper.

MANUFACTURING VARIABILITY AND PRODUCT ASSESSMENT

It will be seen that the preceding arguments depend largely on the degree of variability inherent in manufacturing processes. It is perhaps not surprising that virtually no information on this type of variability can be found in the literature, since such information might be regarded as the private concern of the manufacturing companies. Moreover, unless large numbers of batches are turned out to a single specification annually, it is not an easy matter to obtain a reliable estimate of the manufacturing variability on a process.

An interesting feature which is common to many of the test methods which are employed for inspection purposes is that if a series of test results is obtained on a single batch of product by one operator, and provided that the product remains stable over the period of time in question, and also that the results are spread over a sufficiently long period of time, then their variability will be such as is usually obtained under reproducibility conditions, *i.e.* as if they had all been obtained in different laboratories. It follows that if over such a period of time single results are obtained on a series of batches of nominally identical product, then the variance of these results should be the sum of the manufacturing variance and the reproducibility variance, provided that the test errors are uncorrelated with the " errors " in manufacturing.

Surprisingly, however, if it often found that so far from displaying a variance larger than the reproducibility, such results will often be so remarkably consistent as to differ by no more than would be expected under repeatability conditions. One possible explanation of this phenomenon is as follows. Suppose that against a specification limit S in a particular feature of the inspection data, the manufacturer sets his process so as to turn out batches whose average quality is $S + M$, where M represents the small margin which is imposed so as to ensure that the majority of product is on-grade. In practice, any one batch will depart from his intended quality by some small amount d, so that the true quality of a particular batch may be represented by

$$S + M + d$$

On obtaining a single test result on such a batch a further small error, say e, will be involved, and the observed test result therefore will be

$$S + M + d + e$$

Now if it so happens that $M + d + e$ is negative, so that the test result is below the specification limit, then the batch might be regarded as off-grade and the product might be adjusted, *e.g.* in the case of gasoline by blending in further high octane component or adding lead.

The accumulated background of experience available in respect of such reblending enables the manufacturer to make this adjustment very accurately indeed, and to simplify the argument it may be assumed that he will do it exactly so as to correct the batch quality by the amount $M + d + e$. The true quality of the adjusted batch will then be

$$(S + M + d) - (M + d + e) = S - e$$

In fact, when the manufacturer retests his adjusted batch, and because repeatability of the test procedures is usually very good, the test method will again throw up a result which is an amount (e) too high, so that the new test result on the adjusted product will be precisely (S), and the batch will appear to be just on-grade. But, in fact, the true quality of the batch, which was previously $S + M + d$, is now $S - e$, so that the result of such a procedure might well be to replace the original manufacturing variation represented by variance (d) by the reproducibility variation of the test method, namely, variance (e). Thus, if the variability of the original manufacturing process were less than the reproducibility variance of the test procedure, a product adjustment such as that described above, if applied automatically to all batches, might well lead to worse variation in actual quality than was present before the reblending corrections were made.

At the present time, it has not been possible to demonstrate that the process described above is what actually happens in practice, although it appears to be a plausible explanation for the fact that refinery inspection data on successive batches are often more consistent than would be expected. In making this slightly derogatory statement about refinery inspection data, it must be admitted that the refineries probably use test methods to greater purpose and more frequently than other laboratories, and it might be argued that their greater experience in this respect enables them to achieve better reproducibility than is current elsewhere. Such an assertion, however, is seldom borne out in routine correlation work.

ECONOMICS OF MANUFACTURE

Joint consideration of the economic and statistical aspects of manufacture and inspection testing has not, so far as the author is aware, been attempted previously in the petroleum field, and the remarks which follow are intended merely to indicate the lines which such an approach might take, in the belief that the information so derived may be of some assistance in framing manufacturing policy.

It will be assumed that the basic cost level for pro-

ducing a series of batches to the same specification is that involved in manufacturing exactly to the marketing specification limit and carrying out single inspection tests on each batch, on every inspection feature called for by the specification. Costs additional to this basic level will then arise in a number of ways, namely,

(1) The manufacturer will set his process so as to operate inside the marketing specification in order to avoid an undue amount of reblending or product adjustment. The cost of incorporating such a margin will usually be approximately proportional to the size of the margin, provided that this is not too great.

(2) For any given margin, a certain proportion of the batches produced will in the event give off-grade results, and will hence call for repeat tests and probably, in the majority of cases, for reblending. It is reasonable to assume that the costs so incurred will be proportional to the number of batches "failed" by the initial test results.

(3) Apart from the batches (some good, some bad) which will be "failed" by the initial test results, a certain proportion of the batches which is passed by those results will, in fact, be off-grade and may give rise to justifiable complaint from the customer. This would involve, not merely the reblending of such batches, but also a considerable loss of goodwill to which it is extremely difficult to attach a firm economic value.

The manufacturer's problem consists, then, in striking an appropriate economic balance between the magnitudes of the above three components of additional cost. In order to achieve such a balance he has freedom to fix upon the size of the margin (m), the number of tests which he proposes to carry out on each batch (n), and the position of his acceptance limits in relation to the specification values. In any particular case it will be assumed that the specification limits, the manufacturing variability, and the precision of the test methods which he is required to employ are all fixed.

To simplify the assessment it will be assumed that all batches will be of the same size, so that it is reasonable to assume also that the cost of reblending any one batch is constant, and, moreover, that the economic value in terms of loss of goodwill through having such a batch rejected by the customer is proportional to the number of batches so rejected. The average additional cost per batch made (C) may then be represented by the following formula:

$$C = mM + P_{ba}(R + G) + P_r R + (n - 1)T. \quad (1)$$

where M is the cost per batch per unit margin incorporated in the specification feature involved, P_{ba} is the proportion of bad batches which will on the average be accepted on initial test results, R is the cost of reblending and retesting a batch, G is the economic value in terms of loss of goodwill per off-grade batch passed to the customer, P_r is the proportion of batches made which will on the average be rejected on initial test results, and T is the cost of a single test result.

In general terms, it will be seen that if the manufacturing margin (m) is small, then the corresponding component of cost is small, but the proportion of batches rejected will be large, and in the presence of a poor test procedure so also will be the proportion of bad batches accepted. As against this, if the manufacturing margin is very large, then the proportions of bad batches accepted and of batches rejected will both be small. At either of these ends of the scale the additional cost will be large, and between them there will exist some optimum operating point at which the additional cost is a minimum.

In order to illustrate the sort of situation which may arise in practice, an example has been worked out in terms of arbitrary assumed values for M, G, R, and T, and it has further been assumed that the ratio of manufacturing variability to test method variability (K) is 1 : 1. The values assumed are:

$$T = 1$$
$$M = 20$$
$$B = 200$$
$$G = 1000$$

For a given number (n) of test results on each batch, the ratio of manufacturing variability to the variability of the mean test result is fixed. If, moreover, the manufacturing margin is fixed, then the manufacturer's rejection limit is the only remaining variable. Variation of this limit corresponds to moving along one of the curves illustrated in Figs 5–7. Since $Mm + (n - 1)T$ is regarded as fixed, it is readily seen that the appropriate rejection limit to choose is that which minimizes the additional cost component $P_{ba}(R + G) + P_r R$, and minimum costs may thus be worked out for each of the curves presented in Figs 5–7. Bearing in mind therefore that the appropriate rejection limits are automatically settled by the adoption of these "minimum cost points," the corresponding total additional costs may now be calculated and plotted as functions of both m and n as indicated in Fig 9. It is interesting to note that a true minimum cost is indicated by these data, and it is believed that this will often be the case where practical values of T, M, R, and G are involved.

It is perhaps surprising that when the optimum manufacturing margin of approx 2·4 manufacturing standard deviations is employed, the additional cost is not critically dependent on the number of repeat inspection tests carried out on each batch; a single test is very nearly as "cheap" as the optimum number of three tests per batch. Furthermore, it turns out that in order to achieve the best overall result the manufacturer should set his rejection limit just outside the specification limit, and should in fact pass batches for which the initial test results are just off-specification! This apparently anomalous line of action is justified on the grounds that the test method is of poor

precision and that the process has been set so as to produce less than 1 per cent of bad batches.

It is fully appreciated that the type of analysis described above represents a considerable over-simplification of the actual problems which the manufacturer has to face. No account has been taken of variations in batch sizes or of inter-relation between the economics of one plant or process and the other activities of the refinery. It is nevertheless possible

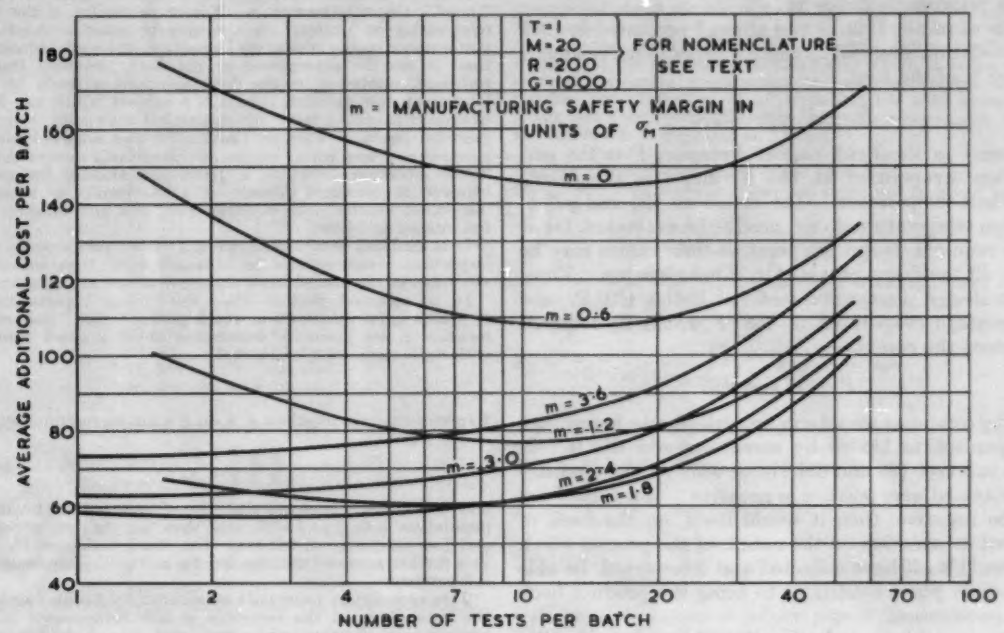

FIG 9

COST EVALUATION OF MANUFACTURING PROCEDURE

that further thought and development along the above lines may lead to useful information on the operation of individual plants or of the refinery as a whole.

SUMMARY AND FUTURE OUTLOOK

It has been shown that the use of test methods which correlate imperfectly with the performance features which it is intended to control, leads to a reduction in refinery flexibility, and is therefore a component cause of increased product cost. Principal offenders in this respect are those inspection data which in fact do not correlate with performance at all, but are incorporated in the specification as a device for ensuring consistency of manufacture or guarding against the risk of contamination or loss in transit or storage.

It may well be that existing knowledge of the correlations which exist between inspection data and field performance is still insufficient to allow the wholesale removal of redundant inspection features from specifications, but this should certainly be the ultimate objective. Contamination and loss could still be guarded against by the mutual exchange of additional inspection data between customer and producer, but such additional data should not be the subject of clauses in the specification which do no more than tie the producer's hand.

Poor precision in test methods, by forcing the manufacturer to work well inside specification, also results in reduced flexibility, and is, moreover, an embarrassment to producer and customer alike in that it leads occasionally to disagreements concerning product quality, many of which are in the event shown to have been unnecessary.

The emphasis which the petroleum industry places on the development of new test methods which correlate directly with performance features is, in the light of the above conclusions, eminently well directed, and the continuing work carried out on the evaluation and improvement of the precision of existing test methods is of first importance. Simplicity should be the key-note of specification, and customers should continue to rely on brand names and the integrity of the oil companies as their best guarantees of quality.

Whether it will ever be possible, completely to define the performance features of a product in quantitative terms by means of correlations with inspection data incorporated in specifications, is a moot point, but if this is ever achieved, it will open up an interesting

vista of fully automatic plant, and even refinery control.

Even if automatic recording of inspection data in plant streams does not keep pace with the derivation of improved correlations between performance and inspection data, it would be possible to feed into existing computing machinery the inspection data on all component streams as such data become available from the laboratory. The problem of briefing electronic calculators on how to handle such correlations presents no difficulty.

For example, Domke and others [3] presented in 1953 a correlation designed to control vapour lock tendency which is of the form :

$$V + P \cdot f(t) < g(t)$$

where V is the Reid Vapour Pressure, P is the percentage evaporated at 158° F, and t is the design ambient temperature. The functions $f(t)$ and $g(t)$ of design temperature do not need to be evaluated, for at any required design temperature their values may be read off the charts provided in Domke's paper. Thus, at a design temperature of 90° F the R.V.P. and percentage evaporated to 158° F would be required to meet the condition :

$$P + 17V < 187$$

The computer would simply multiply the percentage evaporated to 158° F by seventeen, add the R.V.P. and subtract 187 and determine whether the quantity so obtained were positive or negative. If it turned out to be negative, then it would itself, on the basis of stored information on the nature of the process which it would itself have collected and interpreted, be able to adjust plant conditions to bring the product back on specification.

It is possible to go further than this and to visualize a situation where the computer might be instructed to maximize yield or minimize cost of the product subject to the conditions of the specification. Indeed, simple problems of this type have already been solved without the aid of expensive computing equipment, and references to such problems in the petroleum industry are beginning to appear in the literature.[4, 5]

Indulging in even greater flights of fancy, there is no mechanical difficulty about extending the activities of such computing machinery to advice and control over the whole of the refinery's operation. Indeed, it is arguable that any refinery programming which can be done by hand can equally be done by computing machinery, provided only that it is possible to set down explicitly the nature of the programming problem. In the absence, however, of firm correlations and reliable test procedures, possibilities such as the above will necessarily remain as flights of fancy.

References

[1] Kendall, N. *J. Inst. Petrol.*, 1953, **39**, 26.

[2] "Standard Methods for Testing Petroleum and its Products," 14th Ed. London : The Institute of Petroleum, 1955.

[3] Domke, C. J., *et al.* *Petrol. Refin.*, 1953, **32** (7), 151.

[4] Charnes, A., *et al.* *Econometrica*, 1952, **20**, 135.

[5] Symonds, G. H. "Linear Programming is Helpful in Solving Gasoline Refining and Blending Problems." Paper presented to the American Chemical Society Meeting held at Ohio, April 1955.

APPENDIX 1

Correlation Between Field Performance and Inspection Data

Wherever correlation can be shown to exist between a single field performance test and one or more inspection tests, it is of the utmost importance to be able to determine how "good" this relationship is. For in particular, if the true relationship is "exact," then it may be possible to predict performance by the laboratory inspection data more accurately than it can be determined in the field, provided that a sufficient number of repeat determinations is made by the laboratory test methods. This is a subject which has been examined in detail from the theoretical viewpoint over the past ten years, notably by Lindley,[6] whose nomenclature is adopted here and whose results are extensively quoted below.

The situation in which a given performance feature is believed to correlate directly with the results of a single inspection feature only is considered, and this situation is formulated as follows :

It is assumed that transforms η, ζ of the performance and inspection measures can be obtained such that the error variances of the transformed observed values are constant.

It is assumed further that the "true" performance measures η_i for all products which give a "true" inspection measure ξ_i are normally distributed about a mean value $\bar{\eta}_i$ with fixed variance σ_e^2 such that

$$\bar{\eta}_i = \alpha\xi_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Experimental observations x, y on ξ, η are represented by

$$\begin{aligned} x &= \xi + \Delta x \\ y &= \eta + \Delta y \end{aligned} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and it is further assumed that $\Pi(\eta, \xi)$ is a normal bivariate population with $\bar{\xi} = \bar{\eta} = 0$, and that Δx, Δy are normally and independently distributed with fixed variances δ_x^2, δ_y^2. It is further assumed that the Δx, Δy, and $\eta_i - \bar{\eta}_i$ are mutually independent.

This case differs from that considered by Lindley only in the breakdown of the errors in y into components of Δy representing observational error, and $\eta_i - \bar{\eta}_i$ representing imperfect correlation.

It may be shown that

$$\begin{aligned} \sigma_x^2 &= \sigma^2 + \delta_x^2 \\ \sigma_y^2 &= \alpha^2\sigma^2 + (\delta_y^2 + \sigma_e^2) \\ \rho_{xy}\sigma_x\sigma_y &= \alpha\sigma^2 \end{aligned} \quad . \quad . \quad . \quad (3)$$

where σ^2 represents the total variance of ξ.

For a given set of n pairs of observational data, maximum likelihood estimates $\hat{\sigma}_x^2$, $\hat{\sigma}_y^2$ and $\hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y$ may be obtained as

$$\begin{aligned} \hat{\sigma}_x^2 &= (1/\eta)[\Sigma(x - \bar{x})^2] \\ \hat{\sigma}_y^2 &= (1/\eta)[\Sigma(y - \bar{y})^2] \\ \hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y &= (1/\eta)[\Sigma(x - \bar{x})(y - \bar{y})] \end{aligned} \quad . \quad . \quad (4)$$

and if δ_x^2 δ_y^2 are known then a consistent estimate $\hat{\sigma}_e^2$ of σ_e^2 is obtained as

$$\hat{\sigma}_e^2 = \hat{\sigma}_y^2 - \frac{[\hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y]^2}{\hat{\sigma}_x^2 - \delta_x^2} - \delta_y^2 \quad . \quad . \quad . \quad (5)$$

No test of significance is available for $\hat{\sigma}_e^2$, but its magnitude in comparison with δ_y^2 provides a sound means of determining the utility of the correlation.

It may be noted that the variance of a predicted performance measure based on m independent inspection tests is given approximately by

$$\text{var}\,(\eta) = \hat{\sigma}_e^2 + \frac{\alpha^2\delta_x^2}{m}$$

and hence can be made as small as desired by appropriate choice of m, provided that σ_e^2 is zero or suitably small.

As an example, the data represented in Fig 3 (*d*) of the paper were manufactured by assuming

$$\bar{\eta} = \bar{\xi}$$
$$\sigma_e^2 = 0$$
and $$\delta_x^2 = \delta_y^2 = 100$$

Duplicate "observational" results were obtained for both x and y so as to provide estimates of δ_x^2 and δ_y^2, and the correlation analysis is carried out on the mean values X, Y. This gives reduced observational errors

$$\delta^2_X = \tfrac{1}{2}\delta_x^2$$
$$\delta^2_Y = \tfrac{1}{2}\delta_y^2$$

but does not affect the component (σ_e^2) of imperfect correlation. The original data are tabulated below.

"*Observed Data*"

y	x
19, 9	6, 23
13, 4	20, 21
6, 20	29, 15
30, 18	25, 19
27, 28	19, 17
12, 21	38, 25
46, 31	32, 35
32, 43	29, 44
39, 44	45, 39
35, 54	32, 48
39, 65	48, 50
53, 57	67, 54
68, 53	55, 69
72, 58	69, 80
46, 80	70, 88

From these are obtained, in the usual way,

$$\hat{\delta}_y^2 = 120{\cdot}1 \qquad \hat{\delta}_x^2 = 67{\cdot}8 \qquad n = 15$$
$$(1/n)[\Sigma(Y - \bar{Y})^2] = 343{\cdot}97$$
$$(1/n)[\Sigma(X - \bar{X})^2] = 399{\cdot}18$$
$$(1/n)[\Sigma(X - \bar{X})(Y - \bar{Y})] = 345{\cdot}85$$

The estimate of σ_e^2 is then obtained as

$$\hat{\sigma}_e^2 = 343{\cdot}97 - \frac{(345{\cdot}85)^2}{399{\cdot}18 - 67{\cdot}8/2} - 120{\cdot}1/2 \text{ (from equation 5)}$$
$$= -38{\cdot}5$$

The fact that a negative value is obtained indicates clearly that the true σ_e^2 is at worst quite small so that the correlation is eminently satisfactory.

Finally, it may be noted that if it is proposed to predict performance from inspection data of the same precision as that on which the correlation was established, then the usual regression of y on x should be used, in this case

$$y = 2{\cdot}4 + 0{\cdot}866x$$

If, however, it is required to obtain more accurate estimates of performance by carrying out multiple inspection tests so as to improve the precision of the laboratory result, then prediction should be based on the best estimate of the underlying relationship between $\bar{\eta}$ and $\bar{\xi}$. This is

$$(y - \bar{y}) = \hat{\alpha}(x - \bar{x})$$

where $$\hat{\alpha} = \frac{\Sigma(X - \bar{X})(Y - \bar{Y})}{\Sigma(X - \bar{X})^2 - n\delta^2_X}$$

which for the above example turns out to be

$$y = 0{\cdot}947x - 0{\cdot}8$$

compared with the true correlation

$$\eta = \zeta$$

Reference

[6] Lindley, D. V. *J. royal Stat. Soc.*, 1947, Sup. **9** (1–2), 218.

APPENDIX 2

Calculation of Operating Characteristics for Univariate Continuous Variable Inspection Procedures

(1) *Single-limit Case*

Taking the specification limit as origin, it is assumed that the manufacturer sets the process in such a way that the average batch quality is an amount $s\sigma_m$ inside specification, where s is an appropriately chosen safety factor and σ_m is the standard deviation of true batch quality. The true batch qualities, X, are assumed to be normally distributed about a mean value $s\sigma_m$ and with variance σ_m^2.

Single test results on a batch are assumed to be normally distributed about the batch mean X with the reproducibility test variance σ_t^2.

It is also assumed that a batch will automatically be accepted if the first check gives a result which is at least an amount $r\sigma_t$ inside specification, r being so chosen as to regulate the proportion of off-grade batches thus accepted. If the first check result does not meet this requirement, then the batch will be regarded as "failed."

From these assumptions, it follows immediately that the number of on-grade batches when expressed as a proportion (P_G) of the total number of batches made will be given by

$$P_G = \frac{1}{\sqrt{2\pi}\sigma_m}\int_{-s\sigma_m}^{\infty} \exp(-X^2/2\sigma_m^2)dX \quad . \quad (1)$$

that the number of batches accepted when expressed as a proportion P_A of the total number of batches made will be given by

$$P_A = \frac{1}{\sqrt{2\pi}\sqrt{\sigma_t^2 + \sigma_m^2}}\int_{r\sigma_t - s\sigma_m}^{\infty} \exp[-x^2/2(\sigma_t^2 + \sigma_m^2)]dx \quad . \quad (2)$$

and finally, that the number of off-grade batches which will be rejected when expressed as a proportion p_{br} of the total number of batches made, will be given by

$$p_{br} = \frac{1}{\sqrt{2\pi}\sigma_m}\int_{-\infty}^{0} \exp(-(X - s\sigma_m)^2/2\sigma_m^2) \cdot \frac{1}{\sqrt{2\pi}\sigma_t}\int_{-\infty}^{r\sigma_t} \exp(-(x - X)^2/2\sigma_t^2)dxdX \quad . \quad (3)$$

Denoting the integral of the unit normal function from d to infinity by $N(d)$ such that

$$N(d) = \frac{1}{\sqrt{2\pi}}\int_d^{\infty} \exp(-t^2/2)dt$$

then $$P_G = N(-s)$$

and $$P_A = N\left[\frac{r - \theta s}{\sqrt{1 + \theta^2}}\right]$$

where $\theta = \sigma_m/\sigma_t$ is the ratio of manufacturing variability to test method variability.

Further, by substituting

$$v = (X - s\sigma_m)/\sigma_m$$
and $$u = (x - X)/\sigma_t$$

the expression (3) reduces to

$$p_{br} = \frac{1}{2\pi}\int_{-\infty}^{-s} \exp(-v^2/2)\int_{-\infty}^{r - \theta(v+s)} \exp(-u^2/2) \cdot du \cdot dv \quad . \quad (4)$$

Now consider the Normal Bivariate Integral $I_{h,k,\rho}$ denoted by

$$I_{h,k,\rho} = \frac{1}{2\pi\sqrt{1-\rho^2}}\int_{-\infty}^{h}\int_{-\infty}^{k} \exp[-\tfrac{1}{2}(x^2 + v^2 - 2\rho xv)/1 - \rho^2)dx \cdot dv$$
$$= \frac{1}{2\pi\sqrt{1-\rho^2}}\int_{-\infty}^{h}\int_{-\infty}^{k} \exp(-v^2/2) \exp[-\tfrac{1}{2}(x - \rho v)^2/1 - \rho^2]dx \cdot dv$$

By substituting

$$u = (x - \rho v)/\sqrt{1-\rho^2}$$

$$I_{hk\rho} = \frac{1}{2\pi}\int_{-\infty}^{h}\int_{-\infty}^{\frac{k-\rho v}{\sqrt{1-\rho^2}}} \exp(-u^2/2)\exp(-v^2/2)du\,.\,dv. \quad (5)$$

Comparing equations (4) and (5) it is clear that if

$$\frac{k-\rho v}{\sqrt{1-\rho^2}} = r - \theta(v+s) \quad . \quad . \quad . \quad (6)$$

and $$h = -s$$

then $$I_{hk\rho} = p_{bv}$$

Equations (6) require that

$$h = -s$$

$$k = (r - \theta s)/\sqrt{1+\theta^2} \quad . \quad . \quad . \quad (7)$$

and $$\rho = \theta/\sqrt{1+\theta^2}$$

Thus the complete table of proportions accepted and rejected may be derived from

$$P_G = N(h)$$
$$P_A = N(k) \quad . \quad . \quad . \quad . \quad . \quad (8)$$
$$p_{bv} = I_{hk\rho}$$

where the relations between h, k, and ρ, and the basic variables r, s, and θ are as given by equations (7).

Fortunately $I_{hk\rho}$ has been tabulated,* so that no further extensive computation is necessary. For any given r, s, and θ, $I_{hk\rho}$ could be obtained by interpolation in the existing tables, but for the purposes of this paper it has been considered adequate to work directly from the tabulated values of h, k, and ρ and to present the results in graphical form. This accounts in particular for the non-integral values of θ which are represented by the curves presented in the paper.

(2) *Double-limit Case*

In the case of a double-limit specification, it is further assumed that the manufacturer aims to produce an average batch quality exactly mid-way between the specification limits, and this point is taken as origin. This is not necessarily the situation which obtains in practice, since other considerations will generally influence the average position within the specified tolerance, but it nevertheless serves to indicate the performance of test methods under double-limit conditions.

It is assumed that the specification limits are at $\pm s\sigma_m$ and the manufacturer's rejection limits are at $\pm r\sigma_i$ with all other assumptions as for the single-limit case.

The proportion of off-grade batches made is now

$$P_B = \frac{2}{\sqrt{2\pi}\sigma_m}\int_{s\sigma_m}^{\infty} \exp(-X^2/2\sigma_m^2)dx \quad . \quad . \quad (9)$$

the proportion of batches rejected

$$P_R = \frac{2}{\sqrt{2\pi}\sqrt{\sigma_i^2+\sigma_m^2}}\int_{r\sigma_i}^{\infty} \exp[-x^2/2(\sigma_i^2+\sigma_m^2)]dx \quad . \quad (10)$$

and the proportion of bad batches accepted is now given by

$$p_{bs} = \frac{1}{\pi\sigma_m\sigma_i}\int_{-\infty}^{-s\sigma_m} \exp(-X^2/2\sigma_m^2). \int_{-r\sigma_i}^{r\sigma_i} \exp[-(x-X)^2/2\sigma_i^2]dx\,.\,dX \quad . \quad (11)$$

Reducing as before

$$P_B = 2N(-h)$$
$$P_R = 2N(k') \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$
$$p_{bs} = 2[I_{hk'\rho} - I_{h-k'\rho}]$$

where $$k' = \frac{r}{\sqrt{1+\theta^2}}$$

and h, ρ are as for the single-limit situation.

In either case the proportions may be set out in a two-way table as under:

Tested	*Manufactured* On-grade	Off-grade	Overall
Accepted . . .	p_{gs}	p_{bs}	P_A
Rejected . . .	p_{gr}	p_{br}	P_R
Overall . . .	P_G	P_B	1

where

$$p_{gs} + p_{bs} = P_A \qquad P_{gr} + p_{br} = P_R \quad \text{and} \quad P_A + P_R = 1$$
$$p_{bs} + p_{gr} = P_G \qquad p_{bs} + p_{br} = P_B \quad \text{and} \quad P_G + P_B = 1$$

DISCUSSION

R. W. Cranston: Statistics is undoubtedly an aid to specification, and for this reason the title of the paper has been well chosen, thereby putting statistics in its proper perspective.

Mathematical statistics is a relatively new industrial tool and is already proving to be a good servant and friend. There are several different realms in which this is the case; first, in the analysis of results. Workers are realizing that by statistical methods they can obtain the maximum amount of information from their results, and the statistician is being asked with increasing frequency to analyse such results. Sometimes, alas, a mishap occurs in an experiment and the results have little meaning; as a last resort, the worker brings his results to the statistician to see if he can "rescue" them!—which all goes to show that statistics is now acknowledged as a helpful technique for interpreting results.

A second example of the use of this new tool is in the quality control of processes, and a third application which particularly interests me, is the statistical approach to the design of experiments. In industry generally, and in the petroleum industry in particular, it is being realized that statistics has a great deal to offer in the planning of experiments to achieve maximum efficiency.

Mr Sears has focused attention on a further application, namely to specifications, and it is clear after reading his paper and hearing his summary that the industry will benefit greatly by the application of statistics in this field.

The concept of a multidimensional box certainly helps one to visualize the extent to which a set of specifications is imposing restrictions on the manufacturer. It is a very useful qualitative guide and many people will be tempted to apply it rather more quantitatively. There is probably no objection to doing so if one is careful about the scales one employs. For example, Reid Vapour Pressure obviously cannot have negative values, as Mr Sears has pointed out. I should, however, feel happier in considering this property on some such scale as a logarithmic one when negative values are possible. The effects of such transformations of the variables will in general be to give a more open box than has been suggested.

In his paper, Mr Sears refers to non-linear relationships between performance data and inspection data being the rule rather than the exception. I take this to mean that

* "Tables for Statisticians and Biometricians." Vol. II, Tables VIII and IX. Edited by Karl Pearson. Cambridge University Press.

curvilinear relationships are more common than linear ones. Provided, however, that there is a one to one relationship between two properties, I do not agree that the existence of a non-linear relationship between the two properties gives rise to a curved-sided box.

One further point concerning the economics of manufacture. The non-statistical approach is to set the manufacturing target quality sufficiently far inside the sales specification, so that the customer very rarely has cause to reject a batch, *i.e.* by implication the customer's errors of testing are taken into account. The philosophy of this approach is slightly different from that of the usual statistical approach, where the manufacturing target quality is set at such a level that the customer only receives a certain small proportion of off-specification batches. The proportion he actually rejects may be quite different and is not controlled.

Finally, I want to ask if anyone present can from practical experience give quantitative data on the relationship between manufacturing variability and test method variability.

G. W. Sears: Mr Cranston's suggestion that the " box " concept might be turned to quantitative effect is an intriguing one to which I have not as yet given much thought. He is probably correct in thinking that it might be necessary to use transforms, such as the logarithm, of some of the inspection features, but the major difficulty would lie in finding some unifying factor which would determine the choice of scale units for these features or for their transforms, such that the " enclosed " volume would be meaningful.

In this connexion, also, there is a further complication to which I have not alluded in the paper, in that boundaries are imposed by natural correlations over and above those set by the specification limits themselves. Referring to Fig 1, for example, it will be noted that I have plotted in addition to the specification limits, a series of crosses which represent typical motor gasolines. From these it is clear that there is a strong natural correlation between the 10 per cent point and Reid Vapour Pressure in that high vapour pressures are associated with low 10 per cent points, and vice versa. This correlation introduces an additional pair of " parallel " boundaries, the lower of which at least further restricts the working space within the specification by announcing the impracticability of fuels of low vapour pressure and low 10 per cent point. I did not mention this aspect in the paper because I did not wish to confuse the issue by introducing natural correlations between the inspection data in addition to the correlations between inspection data and performance features.

Another point raised by Mr Cranston dealt with the question of curved boundaries to the inspection box. Provided that one to one correspondence exists between an inspection feature and the performance characteristic, then the boundary will be plane whether the correlation is curvilinear or not. My remark in the paper, however, follows on from an assertion that performance features are usually related to more than one piece of inspection data, in which case curved correlations give rise to curved boundaries, and one to one correspondence is automatically ruled out.

On the question of manufacturing variability, it may perhaps be of interest to quote figures which I collected some time ago on the weak mixture ratings of successive batches of 100–130 grade aviation gasoline from one of our refineries. After wading solemnly through a 3-inch thick file of test certificates, I found that I had collected 153 figures of a hundred. I do not propose to dilate tonight on the question of rounding-off, but I would hazard a guess that even if the ratings had been reported to the nearest tenth of an octane number, the variability of the results would still not have given a true reflection of the actual variations in quality that were present.

Finally, I am not sure that I have fully understood Mr Cranston's distinction between the statistical and the non-statistical approach to the setting of specification margins. It is impossible to collect in any reasonable period of time sufficient information on the proportion of actual complaints from the customer, since this will usually be very small, and for this reason it is impossible to set a margin accurately on the basis of this criterion. It would in theory be possible to control manufacture on the basis of the expected number of bad batches which will be rejected, but this is an approach which has not, as far as I know, been employed in the industry. In any event we do not want complaints from customers, and no control based solely on counting complaints would be known to be satisfactory inside a considerable period of time. I believe that the most satisfactory approach may be found in the direction of an attempt at economic assessment of the cost of margins as indicated in the paper.

S. C. Dodson: The author's approach to minimizing cost by forming an equation which has terms representing costs of testing, re-blending, and rejections by customers as well as cost of production is of particular interest. When Mr Cranston mentioned the need for choosing scales carefully with particular reference to Fig 1, it occurred to me that the scales should be arranged to represent equal steps in cost. Thus, with Reid Vapour Pressure it should be possible to estimate the cost of contracting or expanding the limits of a particular product. The relationship between Reid Vapour Pressure and cost will probably be far from linear, so that the scale adjustment will be complex. The estimation of the cost of changing limits is further complicated by the fact that other refinery products must be considered in conjunction with the product under examination. A change in the constitution of one product will affect the constitution of other products also.

However, if such charts are produced for a range of products, the full effect of imperfect setting of specification limits on costs of production will be displayed and should provide a great inducement to all concerned to examine the setting of specifications very carefully.

In the paper, the author has shown how production limits must be contracted because of the errors involved in testing and the existence of imperfect correlations between field and inspection tests.

Another source of variation which he also mentioned, namely, the diversity of the consumer population, may have a great effect on the setting of specification levels.

As an example, consider the anti-knock quality of motor fuels. Fuels are supplied to satisfy a large number of models of car, and in general the fuel must satisfy the consumers whose vehicles require the highest anti-knock quality. Thus, the quality and price are forced up and the majority of consumers pay for the high quality, but derive no benefit from it. This situation is obviously very wasteful, and one tries to compromise at present by producing two or perhaps three different grades of fuel.

There does, of course, exist a co-operative spirit between the motor manufacturers and the petroleum industry, and manufacturers in general aim to produce engines which are satisfied either by an existing premium or regular grade of fuel, but even if all manufacturers designed their engines to have the same requirement of fuel quality, the differences between cars of the same model would have considerable effect.

A few results obtained some time ago illustrate this point. Six cars each of five popular models were tested by an established field procedure to determine the anti-knock quality of fuel required by each car. By that test the pooled estimate of the standard deviation between cars of one model was six octane numbers. This means that if the mean value of the requirement of a very large number of cars of one model was obtained so that this

value could be considered the " true " value, the fuel would have to be marketed at a level ten octane numbers higher to satisfy 95 per cent of the population. Thus, the variation between cars of the same model necessitates a very big jump in the cost of production.

Simple statistical analysis of the results disclosed some further interesting facts. The original tests were carried out in the cars as received, and the engine adjustments were therefore representative of the user population. Further tests were carried out with the distributors adjusted to the maker's recommended setting and again with the setting for maximum power. With the distributors adjusted to the maker's setting the standard deviation was not significantly affected, but at the maximum power setting the standard deviation was reduced to 3·6 octane numbers. This means that the fuel quality has to be only six octane numbers above the mean requirement in order to satisfy 95 per cent of the population.

This example illustrates the effect of population variability in increasing production costs and also the value of the statistical approach in finding underlying causes of variability. In this case, the control of spark advance obviously has a big influence.

The effect on the mean anti-knock requirement of changing the distributor setting has not been discussed above, but to complete the picture I will quote these results. The effect of setting to maximum power condition was to increase the mean anti-knock requirement from 74 octane to 80 octane. However, the marketed fuel octane level would only need to increase from 84 to 90 to satisfy 95 per cent of the population, due to the reduced variability.

G. W. Sears: The use of cost as a unifying factor for the choice of scales for the box is ingenious, but might, I suspect, be complicated by the possibility that the cost of a unit change in vapour pressure, for example, would be dependent on the levels of the other inspection features. A compromise which might partially meet this objection would be to scale in proportion to the average rate of change of cost over the " plane " section of the inspection box at each level of the factor concerned. Mr Dodson also rightly points out that cost in this sense should be assessed in terms of the repercussions of a change throughout the whole of a refinery's products, and this would require multi-variate correlation analyses on all products and in general a great deal more information than is presently available.

The objective is nevertheless very desirable, and given competent estimates of all the correlations involved it is probable that problems of this type could be solved with the aid of electronic computing machinery. A great deal of work undoubtedly remains to be done in the collection of appropriate correlations, however, and I am doubtful whether anything useful could be accomplished even on a single product within the next five years.

W. A. Partridge: As one who for some years has been concerned with writing specifications, I have always been rather appalled by the attitude, general in the industry, that a specification is something that anybody can dash off in their spare time. Obviously, a rather more scientific approach, as Mr Sears has suggested, must be adopted.

It is unfortunate that there are some sordid commercial considerations. These have to be overcome in some way, and I would very much like to have Mr Sears' suggestions in this connexion.

Generally, specifications are a compromise between the marketing side's desire for a perfect product which will sell itself, and the refinery management's need to produce one at minimum cost, with the equipment that they have installed in the refinery, and from the crude oil which they have to handle. Added to this, there are a number of fiscal requirements, namely customs and excise specifications, over which the industry has no control.

Finally, the most important factor is that of competition. It has always seemed to me that within the petroleum industry, which is highly competitive, there is a tendency to use specification points as sales propaganda. This tends, in my experience, to force products to higher quality, at a greatly increased cost, with no apparent benefit to the consumer.

Another difficulty is that of defining satisfactory field performance. As an example, there is the case of the acceptable TEL content in motor gasolines. In some parts of the world, two adjacent areas, presumably using similar types of vehicle, will apparently accept quite markedly different TEL contents. This, no doubt Mr Sears will say, is because there is insufficient correlation between the field performance on TEL content and the manufacturing limits.

An important factor is the impossibility of being really precise, in some cases, as to what constitutes a satisfactory field performance. I have in mind the volatility of motor gasoline. An acceptable volatility, for " warm up " of a motor, for instance, depends to such a great extent on the critical faculty of the user, and that is a thing that I do not think will ever likely be overcome. I have known cases where relatively involatile gasolines have been apparently accepted without complaint, and used quite cheerfully by motorists in one country, whereas in another, with similar climatic conditions, it leads to a series of complaints.

Mr Sears remarked that he found that the consistency of refinery inspection data was higher than he thought it ought to be. I do not find this so difficult to understand, because in the refineries in which I have had some experience it has always been usual for the men on the plant to develop for themselves non-standard tests and criteria, which they can use to control the various streams going to the refinery blend to ensure that it is on specification, and in many cases the product is tested several times at the various stages of production, before it finally reaches the delivery tank. In the case, for example, of aviation gasoline manufacture at Abadan during the war, it was usual to determine the blend composition in the laboratory, to make the blend inside the refinery in the non-leaded state, and then to lead the fluid in the laboratory and test it again. It was then transferred to the lead blending area, where, after transferring, it was tested and finally, after blending, it was again tested. If any further transfers were necessary, more tests were carried out. Thus, multiplicity of testing at various points, coupled with familiarity with one's own product, must produce a higher precision than the sort of precision that one might expect from correlation tests on unknown samples.

Finally, the two major points which Mr Sears has suggested are ones which I, for one, will give very serious consideration. A statistical approach to correlation between field performance and inspection tests is long overdue, but, more than that, from my point of view, the statistical approach to the economics of re-blending, as opposed to taking adequate margins, is something that no one, to my knowledge, has ever really thought about. I have a feeling, however, based entirely on experience, that the amount of re-blending that is done at the present time will be found to be far cheaper than attempting to work to a margin, arrived at statistically.

G. W. Sears: I agree entirely with Mr Partridge on the difficulties inherent in the definition of what constitutes field performance. But such definition is fundamental to the design of a specification; unless the specification is tied directly to field performance, it becomes a mere device for ensuring consistency of product. It is so fundamental in fact that I have taken it as axiomatic not only that field performance can be defined but that it can also be measured.

Provided that this initial step can be taken, then the

subsequent difficulties mentioned by Mr Partridge are comparatively minor. It is true that different correlations between field and inspection data will usually be required for different markets, but experience suggests that correlations for different markets will usually be very nearly parallel so that the same method of control is applicable in all cases. There is reason to believe that this is also true of different levels of customer reaction; if separate correlations are established for trace-knock and for knock-free operation, for example, these will usually be found to be parallel within the limits of experimental error. This being the case, the method of control will be uniquely decided by the correlation used, and the specification limits for a given market may be decided by negotiation between producer and consumer, or, where the specification does not constitute the basis for sale, by empirical determination of the acceptable levels.

Consistency of refinery results is a subject of considerable interest to me, and I have indicated in the paper that I suspect that this consistency may sometimes be spurious. There is a hoary old saying to the effect that figures cannot lie, but that liars can figure. I have no quarrel with the latter, but I should warn you not to put too much reliance on the former. Mr Partridge has made the point that consistency of results on known samples is better than that expected when testing unknown samples. This is quite true of course, but plausible explanations for such a phenomenon are not difficult to find.

Most people are aware, and I have laboured this point in the paper, that many test methods have repeatabilities which are appreciably better than their corresponding reproducibilities. This means in over-simplified terms that if an operator carries out a repeat determination sufficiently quickly after the first and using the same apparatus, etc., then he will obtain the same result. A quicker and cheaper method of obtaining two identical results is to write the second one down as soon as the first determination has been completed. What is more is that since two identical results were going to emerge anyway, the second procedure is just as useful as the first. I am entirely serious about that.

To be fair, however, I must admit that my evidence for believing that the consistency of refinery results is spurious is largely circumstantial, and I do not believe that anyone has indisputable proof one way or the other. It is true that if one can devise within a refinery a non-standard method which is more precise than that laid down in the specification, then this non-standard method may result in improved consistency of actual quality, and hence also, by the arguments which I have outlined, in improved consistency of results by the standard procedure.

It is still a moot point, therefore, whether such consistency of results is spurious or not, and a point, moreover, which could well be settled through the medium of the correlation programmes carried out under the auspices of the Institute.

W. A. Partridge: In regard to the question of cost of working to a margin, I would like to mention a case that I worked on, although I regret not statistically. During the war, in Abadan, when we were manufacturing aviation gasoline, mostly 100–130 grade, production eventually rose to approximately one million tons p.a., and in order to avoid complaint from the A.I.D., the ultimate testing authority, it was found necessary to take half an octane margin over the margin previously set. It was estimated that this margin was, in fact, costing about £600,000 a year.

A. R. Javes: I wonder if Mr Sears could verify a point in the paragraph on page 111 which states, "it may be asked whether the practice of carrying out repeat determinations on batches which are 'failed' on the first test result does not appreciably improve the performance of the test method. The answer to this is unequivocally 'No.'"

Surely there is always the risk of a grossly bad result coming out by accident. Would not that justify making the check determination, in those cases?

G. W. Sears: The statement to which Mr Javes refers is in fact qualified in the following sentence by the remark "rogue results excepted." Even where rogue results are present, however, it should be remembered that a repeat determination will often do no more than repeat the rogue result. In principle, I agree that the practice of carrying out duplicate determinations as a routine precaution is salutary in that it should pick up a greater proportion of rogue results, but my personal view is that in practice the advantages to be gained seldom justify the additional cost of testing.

S. C. Dodson: Do you suggest then that if, after the first sample has failed, a second sample were tested in a different laboratory, either at the same refinery or in an associated establishment, that the procedure would be greatly improved?

G. W. Sears: Quite possibly, but at the expense of some discussion between the laboratories.

C. F. McCue: May I ask Mr Sears a question relating to what he refers to as the "true" value of a characteristic? How does he define this? In the case of characteristics such as lead content or sulphur content, it is obvious what is meant—it is the amount of lead or of sulphur that is present. In some other tests where the spread of results is either side of a mean, then it is reasonable to take the average as being the "true" value. Such a test, I imagine, would be viscosity, where there is no reason why one should get a higher result than a lower. But in some tests the error is always the same way. Vapour pressure is a case in point. It is almost impossible to get a high value for a vapour pressure: all the things that one might do wrong tend to give a low result. In such a case, what is the "true" value?

Mr Sears has referred to correlation programmes—I have never been very happy about the fact that in these programmes the arithmetic mean of the results is almost invariably taken as being the "true" value. It seems to me that with certain tests the highest answer (or maybe the lowest) is more likely to be correct.

G. W. Sears: I think perhaps that Mr McCue is confusing truth with honesty. If a specification requires, for example, that the vapour pressure of a gasoline should be determined by some particular standard method, then it implicitly accepts that that method does measure vapour pressure. For the purposes of the specification, therefore, the true "vapour pressure" is defined as the correct result obtained by application of the method, *i.e.* as the average result that would be obtained if the samples were examined by the method in innumerable different laboratories.

The method may nevertheless be dishonest in the sense that the "vapour pressure" so obtained may bear little relation to the real physical vapour pressure, and if this is the case, then the test method would be described as inaccurate. Even if it is inaccurate, however, the method is sacrosanct in so far as the specification is concerned.

The same distinction applies when analysing the results of a co-operative programme with any test method. If the arithmetic means of the results are used as estimates of the "true" values, then this implicitly accepts the method as being accurate, and leads to an examination only of its precision; this is usually all that is required where IP Standard Methods are concerned. If, on the

other hand, the accuracy of the test method is concerned, then this can be estimated only if there is independent information on the real physical values which the method purports to measure, in which case they may be compared with the corresponding average results obtained by the method so as to give the required information on its accuracy. The precisions of the method are still only measures of the spread of the results about the mean values and bear no relation to the accuracy.

On a small technical point, I should perhaps add that the arithmetic mean is by no means invariably used in the analysis of correlation results. Depending on the nature of the results, it may be preferable to carry out the analysis in terms of transforms, *e.g.* the logarithms or square roots of the results, and the most appropriate mean values are then obtained from the arithmetic means of the transformed results.

C. W. G. Martin: Mr Partridge mentioned that certain operations at Abadan during the war cost £600,000 p.a. in order that an adequate manufacturing margin could be maintained over the specification limits. It is appreciated that this is a lot of money, but, then, one does spend money on insurance premiums.

In presenting the paper, Mr Sears mentioned that no one likes complaints, and these insurance premiums are, in fact, paid in order to reduce the probability of complaints. A complaint can take one of several forms. For example, suppose one is manufacturing a product for which a particular specification limit has been laid down, and that it has a maximum sulphur content of 1 per cent. Further, as a result of some practical trouble, a complaint has been received, a sample has been submitted, tested, and the sulphur content is found to be 1·05 per cent. That does not mean that the complaint is justified. The engine probably has not the faintest idea whether the sulphur is 1, 1·05, or 1·5 per cent—the engine's "indigestion" is probably due to something quite different. It is merely by sheer mischance that the lodging of the complaint has also uncovered the possibility that the sample did not contain less than 1 per cent sulphur as required by specification.

Alternatively, the complaint can be of another kind. Suppose one is responsible for shipping a cargo to a mythical territory where the Customs authorities will not accept a sulphur content higher than 1 per cent. It turns out on analysis to have 1·05 per cent of sulphur and one just cannot convince these difficult Customs authorities that the sulphur level really was 1 per cent or less, and so one has to take the cargo away. If that sort of thing happens very often, one quickly runs up a bill which more than swamps any insurance premium which has had to be paid in order to meet the admittedly arbitrary margin (and in many cases it has, up to now, been quite arbitrary) which is selected when producing a so-called manufacturing specification.

In the first case that I have cited, where the engine ran into trouble, there was no question of any manufacturing margin. The engine's "indigestion" was quite fortuitous, and so long as successive supplies were not far above or below the target of 1 per cent sulphur, it is most unlikely that changes in sulphur *per se* would even be noticed by the apparatus.

The second case is one of a quite different kind, and it becomes the statistician's problem to assess that margin which one must work to in such obligatory circumstances.

There is a very great deal of money involved in the selection of adequate, but still economic, safety margins in specifications. Consequently, the author's reminder regarding the necessity for improving the precision of test methods and, for that matter, the consistency of any reagents which are used in the methods is very timely.

D. Rawlinson: It seems to me that the logical outcome of this discussion is almost to do away with laboratory testing altogether. Mr Sears in a rather humorous way appears to cast doubts on the value of the laboratory results; doubts which are shared by some people outside the laboratories. I would like, therefore, to put some practical thoughts about this.

Following up what Mr Sears has suggested, one needs specifications which will guarantee to the user that the product which he gets will do what he wants. To do this, it is suggested that it is desirable to know more about manufacturing variability. If one can obtain sufficient information about this variability and then set the process to produce within defined limits, the producers would feel quite happy to say that for a given feedstock under defined process conditions with known variability, the product will be satisfactory in field performance. In that case, it should be possible to agree beforehand with the customer that laboratory testing is unnecessary, except, perhaps, at the beginning of a process.

A previous speaker has referred to frequency of testing as a guarantee that the product will be on specification, and has suggested that repeated testing could explain why Mr Sears found 153 values of exactly 100 for an octane number. This again is a matter where I wonder if statistics might help to indicate how much time is wasted in laboratories on repeat testing. A test is made every time somebody transfers a bucketful in or out of a tank containing 3500 tons.

Statistically, tests in these circumstances would seem to mean very little. The product is transferred through a pipeline, and unless somebody leaves a valve open, the actual quality of the material cannot really be altered, in spite of a different laboratory result. For example, if a fuel with a sulphur content of 2·4 is transferred through a pipeline, and a subsequent result is 2·5, one knows that the quality cannot have changed by that amount barring gross errors. If the possibility of gross errors is much smaller on the refinery than in the test itself, it would seem pointless to carry out the check test. Finally, on this question of whether anything is achieved by a repeat determination on a sample, I would like to know what Mr Sears thinks of the value of drawing another sample. I raise this point because whatever the man in the laboratory may feel statistically, he will, in fact, have to accept the responsibility of passing or rejecting material. To reject a batch may incur considerable expenditure on re-processing (other speakers have referred to large amounts of money involved), and the laboratory chemist may be held responsible for this expenditure. In self-defence therefore, he has to consider further testing, either of the same sample in case a mistake has been made in the first test, or of a repeat sample, bearing in mind that the first one may not be representative. When one thinks of the difficulties of sampling at night in bad weather, and the fact that a 5000-ton tank may be represented by three 1-qt cans this is not at all impossible.

On the other hand, there is a well-known rule in refineries that one keeps on sending samples until the laboratory produces the right result.

G. W. Sears: Never having worked in a refinery, I cannot vouch for the prevalence of the practice mentioned by Mr Rawlinson. Presumably, it would serve to ensure consistency of results, but I would imagine that it is more expensive than the simple copying process to which I referred earlier.

I agree entirely that the logical outcome of the thoughts which I have put before you might be to abandon an appreciable amount of the inspection testing which is presently applied to ensure conformity with specification. I have reason to believe that there are several test methods in general use which, far from being effective in controlling product quality, may actually be making matters worse through the application of unwarranted re-blending. Such methods should at most be used as a check against gross errors in manufacture. It is possible

therefore that a critical examination of the whole of a refinery's testing programme might lead to the elimination of a fair amount of unnecessary work; equally it might indicate several points at which additional checks were desirable so that the result of such an examination might be better assurance regarding product quality rather than an appreciable saving in inspection cost.

The question of the utility of drawing additional samples is not one with which I can deal adequately in a short answer. Indeed, the matter was examined at some length by the Statistical Sub-Committee at the request of the Sampling Panel some years ago. In general terms, however, it will be clear that if the contents of a tank are homogeneous, then all samples from the tank will be identical, and no useful purpose can be served by drawing more than one sample. At the other end of the scale, if there is a considerable variation in quality between different samples from the tank, then no useful purpose is served by carrying out repeat tests on any one sample, for this would lead to an improvement in precision on a sample which was unlikely to be representative anyway.

The sampling problem has in fact a great deal in common with the main issues discussed in this paper, and a satisfactory solution can be arrived at only by taking economic factors into account. The best number of samples to take, and the best number of tests to carry out on the bulked sample both depend on the ratio of the test method precision to the sample, to sample variability, on the relative cost of carrying out an inspection test, and of drawing a further sample.

Sampling a product which has been drummed off, which was the problem referred to us by the Sampling Panel, is further complicated by the possibility of contamination or loss in individual drums, but this is outside the scope of the present question.

P. G. Layzell: There are two points I should like to raise with Mr Sears.

The first concerns the problem of determining the relationship between manufacturing and testing error. My laboratory has done a little work on this problem relating to a subject which has been much discussed, *i.e.* the TEL content of gasoline. With the help of our Statistics Department we designed an experiment which separated the standard deviations of the blending procedure and testing. This was a fairly easy problem because we were weighing a pre-determined quantity of TEL fluid into each batch and we knew exactly what concentrations we were aiming at. Most refinery problems, however, are more complex, and I consider it is much more difficult to study, for example, the variation in the initial boiling point of a distillation tower sidestream. There may be no deliberate attempt to change this property, but there might easily be a systematic error brought about by changes made deliberately to the adjacent stream.

The other point concerns Mr Sears' view on "repeat tests," which certainly confirms my suspicions of statisticians.

Mr Sears chose, as we all would in the circumstances, an example which favours his own point of view, *i.e.* octane number. It is a good example because octane engines are expensive and there are not many laboratories with more than about four octane engines. This does not apply to other tests. When a sample is "repeat tested" it is probably tested by a different tester on a different shift, quite possibly with different apparatus, and I am sure that the mean of such results is more valuable than any single determination. If Mr Sears seriously disagrees with me, it is difficult to understand his basis for defining earlier a "true value" as a mean of a number of determinations.

G. W. Sears: I am afraid that I must plead guilty to having simplified the issues involved by generalization, and I am sorry if this has added fuel to Mr Layzell's suspicions of statisticians. I have defined a "true" value as being the mean of the results from innumerable laboratories; this is an hypothetical concept which, as Mr Layzell rightly deduces, implies that given a sufficient number of independent results, then an absolutely precise value will be obtained. But this does not in any way conflict with my assertion that one or two repeat determinations, which will usually not be independent, do not very materially improve the precision of the mean result.

To particularize; if a test method has a reproducibility which is twice as large as its repeatability, a situation which is quite common among IP Standard Methods, then the precision of the mean of two determinations by the same operator is only about six per cent better than that of a single determination. Whereas it is quite true therefore that two results are necessarily better than one, it is often the case that the gain from the second result is too small to justify the cost of the repeat determination.

With some methods a much more marked improvement in precision can be obtained by using different apparatus or a different tester, but this is by no means universal, and with other methods the improvement is no better than that obtained with a single operator and apparatus. The methods with which one has to deal are so diverse in their character and behaviour that no generalizations are wholly satisfactory.

I am not sure that I have fully grasped the distinction made by Mr Layzell in his first point. Unless it is possible to be absolutely sure that the blended product remains homogeneous and loss-free in transit from the blending tanks to the customer's storage, then calculated concentrations of TEL are an unreliable guide to actual manufacturing variability. Whether the investigation be into TEL contents or initial boiling points, therefore, I believe that one is forced into estimating manufacturing variability from the analytical results on successive samples from a series of batches. This is straightforward enough if the errors in the analytical results are independent of the variations in product quality, but I have indicated in the paper that there are reasons for believing that this will not always be the case.

When the analytical results are not independent of variations in product quality the assessment of manufacturing variability may be much more difficult. It is this natural ability for figures to lie that sustains me in my reciprocal suspicion of refinery technologists and their problems.

The meeting then closed with a unanimous vote of thanks to the author.

AN ALTERNATIVE APPROACH TO THE DESIGN OF 1–2 HEAT EXCHANGERS *

By E. T. NELSON †

SUMMARY

An alternative method is presented for the design of 1–2 heat exchangers which, for constant overall heat transfer coefficient, leads to an expression for the mean temperature difference in the familiar logarithmic mean form. Where the overall heat transfer coefficient may be assumed to be a linear function of temperature, the method provides an exact treatment which has hitherto been impossible owing to the mathematical complexity of the established method involving a LMTD correction factor.

An example is given for the simple case of a 1–2 heat exchanger with constant overall coefficient, and a second example has been presented to illustrate the method when applied under conditions of controlling shell-side coefficient.

NOMENCLATURE

Defining the temperatures involved by Figs 1 and 2, and using any set of self-consistent units :

W and w = mass flow rates of fluids entering at T_1 and t_1 respectively,
C and c = specific heat of fluids entering at T_1 and t_1 respectively,
A = total area for heat transfer,
a = total area per unit length of exchanger,
a' and a'' = total area per unit length of tube pass,
U_1 = overall heat transfer coefficient when $t' = t_1$,
U_2 = overall heat transfer coefficient when $t'' = t_2$,
and U_c = overall heat transfer coefficient when $t' = t'' = t_c$.

INTRODUCTION

A DESIGN method capable of dealing with variable overall heat transfer coefficients in 1–2 heat exchangers has been required by petroleum technologists for some time owing to the rapidity with which the heat transfer properties of the less volatile petroleum fractions change with temperature. Gardner [5] published an approximate method for obtaining the mean temperature difference correction factor in 1–2 heat exchangers with the tube-side film controlling, and the same author [6] derived expressions for the case of the shell-side film controlling. However, there appears to be no method which may be applied when neither fluid film may be said to control. It is the purpose of this paper to present an alternative approach to the design of 1–2 heat exchangers which may be used when the overall coefficient is assumed to be a linear function of temperature and when neither fluid film controls.

A mathematical derivation of the correction factor by which the LMTD for counterflow must be multiplied to give the true mean temperature difference for 1–2 heat exchangers was first presented by Nagle.[1] The trial and error solution involved was shown by Underwood [2] to be avoidable. Underwood succeeded in integrating the equations involved and presented formulæ giving the true mean temperature difference in terms of the terminal temperatures. Later Bowman, Mueller, and Nagle [3] co-ordinated the results of these and other investigators, and presented the method in the form in which it now appears in most heat transfer textbooks. Though other authors have contributed towards the extension of these analyses, the derivation due to Nagle and Underwood has remained the basis of 1–2 heat exchanger design for more than twenty years.

This method, which uses only elementary mathematics to derive the final relationships, is introduced

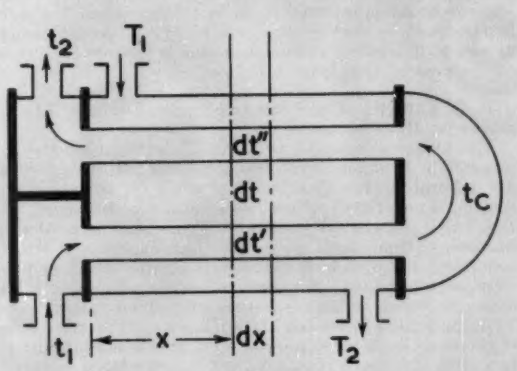

FIG 1

as case I by considering the simple 1–2 heat exchanger of Fig 1, and applying the same assumptions as quoted by Bowman, Mueller, and Nagle :

(1) The overall heat transfer coefficient U is constant throughout the heat exchanger.
(2) The rate of flow of each fluid is constant.
(3) The specific heat of each fluid is constant.
(4) There is no condensation of vapour or boiling of liquid in part of the exchanger.
(5) Heat losses are negligible.
(6) There is equal heat transfer surface in each pass.
(7) The temperature of the shell-side fluid in any shell-side pass is uniform over any cross-section.

* MS received 6 January 1956.

† London Research Station, The Gas Council.

By using the same assumption as that used by Colburn [4] for the 1–1 heat exchanger, *i.e.* the linear variation of U with temperature, and by assuming unequal areas per pass, it is possible to derive relationships in which assumptions (1) and (6) are eliminated by the method which is presented as case II.

CASE I

1–2 *Exchanger with Equal Heating Surface per Pass and Constant Overall Coefficient*

By assuming constant specific heats and mass flow rates, the heat content q of the shell-side fluid may be expressed as a linear function of its temperature, *i.e.* $dq = WCdt$. In addition, in view of the heat balance relationship over the section dx, the temperatures, and consequently the temperature differences, are all

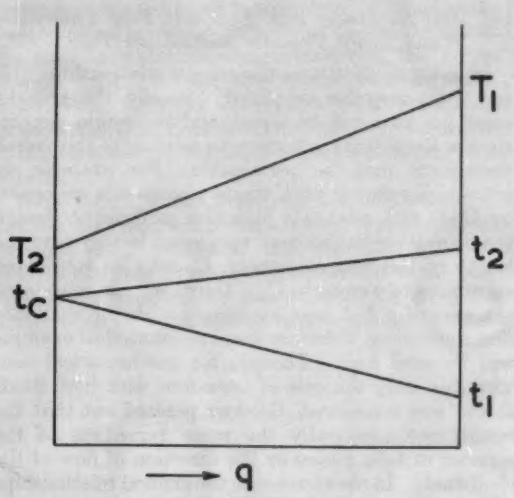

FIG 2

linearly related to q as shown in Fig 2. If the hot fluid temperature is t at a point distance x from the inlet and the remaining terminal temperatures are as shown in Fig 1, the appropriate temperature differences may be expressed as linear functions of t as follows:

When $t = T_1\ \Delta t'' = T_1 - t_2$

$t = T_2\ \Delta t'' = T_2 - t_c$

Hence

$$\Delta t'' = \frac{(T_1 - t_2) - (T_2 - t_c)}{T_1 - T_2}(t - T_2) + (T_2 - t_c) \quad (1)$$

When $t = T_1\ \Delta t' = T_1 - t_1$

$t = T_2\ \Delta t' = T_2 - t_c$

Hence

$$\Delta t' = \frac{(T_1 - t_1) - (T_2 - t_c)}{T_1 - T_2}(t - T_2) + (T_2 - t_c) \quad (2)$$

A heat balance over the section dx gives:

$$-WCdt - wcdt'' + wcdt' = 0 \quad . \quad . \quad (3)$$

Rate equations for the two passes are:

$$-wcdt'' = U(\tfrac{1}{2}a)dx\Delta t'' \quad . \quad . \quad . \quad (4)$$

$$wcdt' = U(\tfrac{1}{2}a)dx\Delta t' \quad . \quad . \quad . \quad (5)$$

Combining equations (3) to (5)

$$WCdt = U(\tfrac{1}{2}a)dx\Delta t' + U(\tfrac{1}{2}a)dx\Delta t''$$

Hence

$$\int_{T_1}^{T_2} \frac{dt}{\Delta t'' + \Delta t'} = \frac{1}{WC}\int_0^L \tfrac{1}{2}(Uadx)$$

Substituting for $\Delta t''$ and $\Delta t'$ and integrating, the true mean temperature driving force is obtained in the familiar logarithmic mean form:

$$WC(T_1 - T_2) = UA\left[\left(T_1 - \frac{t_1 + t_2}{2}\right) - (T_2 - t_c)\right] \Big/ \ln\left[\left(T_1 - \frac{t_1 + t_2}{2}\right) \Big/ (T_2 - t_c)\right] \quad (6)$$

i.e. for case I assumptions the true mean temperature difference is the logarithmic mean of the arithmetic mean driving forces at each end of the heat exchanger.

Before equation (6) may be used, another relationship is required between t_c and the remaining terminal temperatures. This may be obtained from the rate equations for the two tube passes as follows. By expressing $\Delta t''$ as a function of t' and $\Delta t'$ as a function of t'' we have:

$$\Delta t'' = \frac{(T_1 - t_2) - (T_2 - t_c)}{(t_1 - t_c)}(t' - t_c) + (T_2 - t_c) \quad (7)$$

$$\Delta t' = \frac{(T_1 - t_1) - (T_2 - t_c)}{(t_2 - t_c)}(t'' - t_c) + (T_2 - t_c) \quad (8)$$

From equations (4) and (5)

$$\int_{t_1}^{t_c} \Delta t' dt'' + \int_{t_1}^{t_c} \Delta t'' dt' = 0 \quad . \quad . \quad (9)$$

The combination of equations (7) to (9) gives

$$t_2 = \frac{(T_1 + T_2)(2t_c - t_1) - 2t_c^2}{T_1 + T_2 - 2t_1} \quad . \quad (10)$$

which when solved for t_c gives

$$t_c = \tfrac{1}{2}\{T_1 + T_2 - \sqrt{(T_1 + T_2)^2 - 4[\tfrac{1}{2}(t_1 + t_2)(T_1 + T_2) - t_1 t_2]}\} \quad (11)$$

Example 1. Assuming the temperature distribution $T_1 = 350$, $T_2 = 200$, $t_1 = 80$, and $t_2 = 190$, calculate the true mean temperature difference for the heat exchanger of Fig 1. (*a*) By means of the method of Bowman, Mueller, and Nagle [3] and (*b*) by using equations (6) and (11).

(*a*) $R = (T_1 - T_2)/(t_2 - t_1) = 1{\cdot}364$

$P = (t_2 - t_1)/(T_1 - t_1) = 0{\cdot}407$

$$F = \frac{\sqrt{R^2 + 1}}{R - 1} \ln \frac{1 - P}{1 - PR} \Big/ \ln \frac{(2/P) - 1 - R + \sqrt{R^2 + 1}}{(2/P) - 1 - R - \sqrt{R^2 + 1}} = 0{\cdot}837$$

LMTD =
$$[(T_1 - t_2) - (T_2 - t_1)]/\ln[(T_1 - t_2)/(T_2 - t_1)] = 139$$

$\therefore$ True mean temperature difference =
$$F \times \text{LMTD} = 116$$

(*b*) $t_c = \frac{1}{2}\{T_1 + T_2 - \sqrt{(T_1 + T_2)^2 - 4[\frac{1}{2}(t_1 + t_2)(T_1 + T_2) - t_1 t_2]}\} = 146$

$\therefore$ True mean temperature difference =
$$\left[\left(T_1 - \frac{t_1 + t_2}{2}\right) - (T_2 - t_c)\right] / \ln\left[\left(T_1 - \frac{t_1 + t_2}{2}\right)/(T_2 - t_c)\right] = 116$$

CASE II

1–2 *Exchanger with Unequal Heating Surface per Pass Assuming the Variation of* U *to be Linear with Temperature*

In this case equations (4) and (5) become

$$-wcdt'' = U''a''dx\Delta t'' \quad . \quad . \quad (12)$$
$$wcdt' = U'a'dx\Delta t' \quad . \quad . \quad . \quad (13)$$

An expression analogous to equation (6) may be arrived at by expressing U and Δt for each pass as linear functions of t. This results in a quadratic function of t for each of the products $U'a\Delta't'$ and $U''a''\Delta t''$, which may be combined to give

$$U'a'\Delta t' + U''a''\Delta t'' = A_1 + A_2\left(\frac{t - T_2}{T_1 - T_2}\right) + A_3\left(\frac{t - T_2}{T_1 - T_2}\right)^2 \quad (14)$$

where

$$A_1 = (T_2 - t_c)Uc(a' + a'')$$
$$A_2 = (T_2 - t_c)[(U_1a' + U_2a'') - 2Uc(a' + a'')] + Uc[a'(T_1 - t_1) + a''(T_1 - t_2)]$$
$$A_3 = (T_2 - t_c)[U_c(a' + a'') - (U_1a' + U_2a'')] + (T_1 - t_1)(U_1 - U_c)a' + (T_1 - t_2)(U_2 - U_c)a''$$

Using equations (12) and (13), the differential heat balance becomes

$$WCdt = U'a'dx\Delta t' + U''a''dx\Delta t''$$

which on substituting for $U'a'\Delta t' + U''a''\Delta t''$ from equation (14) leads to

$$\int_{T_1}^{T_2} \frac{dt}{A_1 + A_2\left(\frac{t - T_2}{T_1 - T_2}\right) + A_3\left(\frac{t - T_2}{T_1 - T_2}\right)^2} = \frac{1}{WC}\int_0^L dx$$

Upon integration this gives the required relationship

$$(T_1 - T_2)WC = L\sqrt{A_2^2 - 4A_1A_3} \cdot \left[\ln\frac{\frac{1}{2}(A_2 + \sqrt{A_2^2 - 4A_1A_3}) + A_1}{\frac{1}{2}(A_2 - \sqrt{A_2^2 - 4A_1A_3}) + A_1}\right]^{-1} \quad (15)$$

The relationship between t_c and the terminal temperatures may be obtained by expressing $U'a'\Delta t'$ as a quadratic function of t'' and $U''a''\Delta t''$ as a quadratic function of t'.

From the rate equations (12) and (13)

$$\int_{t_2}^{t_c} U'a'\Delta t'dt'' + \int_{t_1}^{t_c} U''a''\Delta t''dt' = 0$$

hence, substituting for $U'a'\Delta t'$ and $U''a''\Delta t''$ and integrating, the equation corresponding to equation (10) becomes

$$t_2 = \frac{\begin{array}{c}T_1[t_c(2a_1 + a_2) - a_4t_1] + \\ T_2[t_c(2a_2 + a_1) - a_3t_1] - \\ (2a_2 + a_1)t_c^2 + (a_3 - a_5)t_1t_c\end{array}}{a_5T_1 + a_6T_2 - (2a_1 + a_2)t_1 + (a_4 - a_6)t_c} \quad (16)$$

where $a_1 = U_1a' + U_2a''$ $\quad a_4 = (2U_2 + U_c)a''$
$a_2 = U_c(a' + a'')$ $\quad a_5 = (2U_1 + U_c)a'$
$a_3 = (2U_c + U_2)a''$ $\quad a_6 = (2U_c + U_1)a'$

CASE III

1–2 *Heat Exchanger with Shell-side Film Controlling and Equal Transfer Surface per Pass*

In many applications, the general relationships, (15) and (16), may be simplified; usually the transfer areas per pass will be equal, and in certain circumstances simplifying assumptions relating to the overall coefficients may be permissible. For example, in petroleum refinery work where viscous oils are cooled by fluids with relatively high film coefficients, though the overall coefficient may vary considerably over the length of the heat exchanger, U_1 may be considered approximately equal to U_2. Gardner [6] has considered the case of a 1–2 heat exchanger with the shell-side film controlling, therefore the same numerical example will be used here. Though, for mathematical convenience, only the case of cross-flow with both fluids mixed was considered, Gardner pointed out that the results are practically the same regardless of the number of tube passes or the direction of flow of the shell fluid. In the above case the general relationships of case II may be simplified as follows:

When $a' = a'' = (\frac{1}{2}a)$ and $U_1 = U_2 = U_0$ equation (16) becomes

$$t_2 = \frac{(RT_1 + T_2)(2t_c - t_1) - 2t_c^2 - (R - 1)t_1t_c}{RT_1 + T_2 - 2Rt_1 + (R - 1)t_c} \quad (17)$$

where $R = (2U_0 + U_c)/(2U_c + U_0)$.

Equation (15) may be simplified by substituting A for L, and by replacing A_1, A_2, and A_3 by A_1', A_2', and A_3'

where $A_1' = U_c(T_2 - t_c)$

$$A_2' = U_c\left(T_1 - \frac{t_1 + t_2}{2}\right) - (2U_c - U_0)(T_2 - t_c)$$

$$A_3' = (U_0 - U_c)\left[\left(T_1 - \frac{t_1 + t_2}{2}\right) - (T_2 - t_c)\right]$$

Example 2. Calculate the mean rate-temperature difference ($U\Delta t$)m given that $T_1 = 520°$ F, $T_2 = 220°$ F, $t_1 = 100°$ F, $t_2 = 200°$ F, $U_1 = U_2 = 96$ B.t.u/hr.ft^2 °F and $U_c = 32$ B.t.u/hr.ft^2 °F.

$R = 1{\cdot}4$ and equation (17) gives $t_c = 156°$ F.

From equation (15)

$$(U\triangle t)m = \sqrt{A_2'^2 - 4A_1'A_3'} \left[\ln \frac{\frac{1}{2}(A_2' + \sqrt{A_2'^2 - 4A_1'A_3'}) + A_1'}{\frac{1}{2}(A_2' - \sqrt{A_2'^2 - 4A_1'A_3'}) + A_1'}\right]^{-1}$$
$$= 8680 \text{ B.t.u/hr.ft}^2.$$

This agrees with the correction factor of 0·665 given by Gardner,[6] *i.e.* arithmetic mean of U_0 and $U_c = 64$ B.t.u/hr.ft²° F. Logarithmic mean of $(T_1 - t_2)$ and $(T_2 - t_1) = 204°$ F.

$\therefore (U\triangle t)m = 64 \times 204 \times 0{\cdot}665 = 8680$ B.t.u/hr.ft².

CONCLUSION

Although the method described in this paper has the obvious disadvantage that for multipass heat exchangers an unknown temperature is introduced at each terminal temperature between passes, the method has many advantages when considering the 1–2 exchanger only. In the first place, the elementary mathematics involved will no doubt find favour with those students of heat transfer who find the solution of Underwood's differential equations rather complicated.

With regard to the design of 1–2 exchangers, the method described will provide a more logical approach for some design problems. For example, in the case where a hot fluid entering countercurrent to the outlet coolant is required to be cooled through a given temperature range by means of readily available cooling water, it is more natural to select a reasonable approach $(T_2 - t_c)$ based on previous experience, than to try a cooling water outlet temperature (t_2) and to see if this results in a reasonable F factor. Equations (10) and (16) have been presented with t_2 as the subject of the formulæ with this application in mind; in addition, since U_c in equation (16) will, in general, be a function of t_c, it would be impracticable to solve this equation with t_c expressed explicitly in terms of the other terminal temperatures.

The author's chief intention in this paper is to present an alternative approach to the design of 1–2 heat exchangers which may be applied under conditions of varying overall coefficient. Though only one type of heat exchanger has been considered, it is hoped that this paper will lead to the extension of the method to other multipass heat exchangers.

ACKNOWLEDGMENTS

The author wishes to thank the Gas Council and the North Thames Gas Board for permission to publish this paper. Thanks are also due to R. J. Wingrove for checking the mathematics.

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OBITUARY

ALEXANDER FRANK DABELL

1873–1956

By the death on 25 February 1956 of Alexander Frank Dabell the petroleum industry loses another of its pioneers. Trained as an engineer in the Southampton naval works and at the Fairfield Shipbuilding and Engineering Company's works in Glasgow, he served two years as a marine engineer, part of his service being in tankers, and qualified for his 1st Class certificate.

Following his sea service, Frank Dabell joined the European Petroleum Company as chief engineer at their Thames Haven refinery, the first oil refinery in England. He then went to Rumania and was engaged in well drilling with the first rotary outfit used in that country. His next move was in 1904 to Turkey, to drill there the first well in search for oil.

From Turkey he went to Baku as field manager to the Baku Russian Petroleum Company, then to Maikop as general manager to the Maikop Corporation, and on to Grosny in a similar post with the Spies Petroleum Company. The latter post he occupied both during and after the Russian revolution. After leaving Russia he went back to Rumania as chief engineer to the Anglo-French Petroleum Commission. From Rumania he proceeded to Trinidad, where he became general manager to British Controlled Oilfields and to Trinidad Petroleum Development Company until his retirement.

After his return to England and retirement he became particularly interested in local affairs in Huntingdonshire, and served on his borough council and on various local committees. Among his activities he was president of the Huntingdonshire Bee-keepers Association and was ARP Divisional Director in his area.

As one of those who agreed to become a member of the Institute before its foundation as the Institution of Petroleum Technologists, Frank Dabell always took a keen interest in its affairs. For a number of years he was a Member of Council and was largely instrumental in forming the Rumanian and Trinidad Branches, of both of which he was the first chairman.

He held the Order of Chevalier de la Couronne du Rumanie.

Alexander Frank Dabell was known to many in the petroleum industry as a good counsellor and a valued friend.

G. S.

CORRESPONDENCE

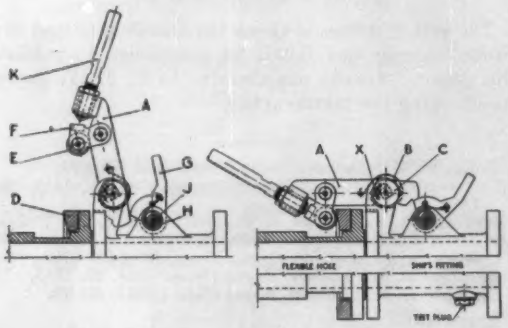
MODERN CRUDE OIL LOADING TECHNIQUES AT MINA-AL-AHMADI, KUWAIT

The Editor, *Journal of the Institute of Petroleum.*

SIR,

Having read with much interest the above-named paper by J. M. Dougary published in the January issue of your *Journal*, I should like to make the following comments.

I note in the discussion that L. J. Shrubsall raised points regarding the emptying of hoses before disconnecting after loading and also regarding emergency release snap couplings.



CLAMPED PIPE COUPLING

B.P. 675733

A—pressure arm; B—pressure arm spring; C—pressure arm fulcrum pin; D—pressure ring; E—pressure ring roller; F—pressure ring roller carrier; G—trigger arm; H—trigger arm spring; J—trigger arm fulcrum pin; K—operating lever; X—set bolt to secure fulcrum pin (C).

In reply to the latter question Mr Dougary stated that as a normal rule the loading hoses are connected by bolts and during unfavourable weather by bolted clamps.

It is felt in many quarters that the bolting up of ship to shore hose connexions is far from satisfactory, and I would like to point out that "Shell Tankers" have for some time appreciated this. They have now adopted as a standard fitting on their ships a clamped pipe coupling which overcomes the disadvantages of the bolted connexion and at the same time offers a reliable and easy quick-release in an emergency (see accompanying Figure).

Yours truly,

for THE SHELL PETROLEUM CO. LTD.,

London, JAS. W. STRANG

23 *January* 1956.

The Editor, *Journal of the Institute of Petroleum.*

SIR,

I have noted with interest Jas. W. Strang's comments regarding the use of the clamped pipe coupling as fitted to "Shell Tankers" vessels, and have discussed the merits of the clamping device with our marine operating personnel.

The consensus of opinion is, that while the coupling certainly does what is claimed for it in providing a reliable and quick release in an emergency, in general it adds little to the operating efficiency of the Mina terminal.

The major drawback at the present time, which is one which could be easily remedied, is that the clamped pipe coupling is provided for 8-inch dia hoses, whereas the majority of our loading berths are fitted with 10-inch loading hoses. This means that we have to fit an 8/10-inch reducer to the clamped pipe coupling and then bolt up the 10-inch hose to the 10-inch end of the reducer, thus little time is saved in the hosing up operation. In any case, with the vast majority of ships using the terminal, including "Shell Tankers" vessels, requiring 4 hours and upwards for deballasting, the time involved in bolting up is not lost time for either the vessel or the terminal.

In normal circumstances when the loading hoses are connected by bolted connexions, only one bolt in two or three in the flange is connected up, and the time occupied is normally not more than $\frac{3}{4}$ hour.

Our clamped connexions can be fitted in even shorter time, and in time of emergency, can be knocked from the flanges in a matter of a few seconds.

Yours faithfully,

Kuwait, J. M. DOUGARY

18 *February* 1956.

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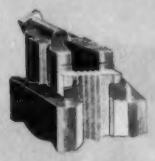

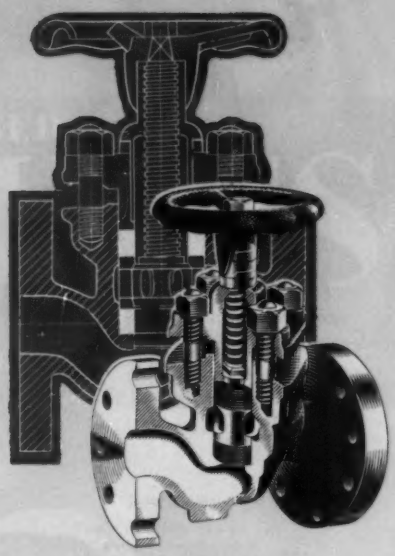

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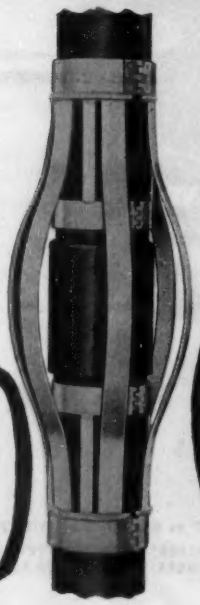
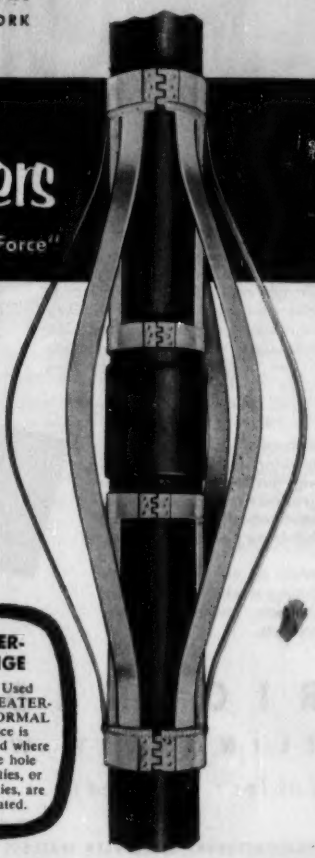
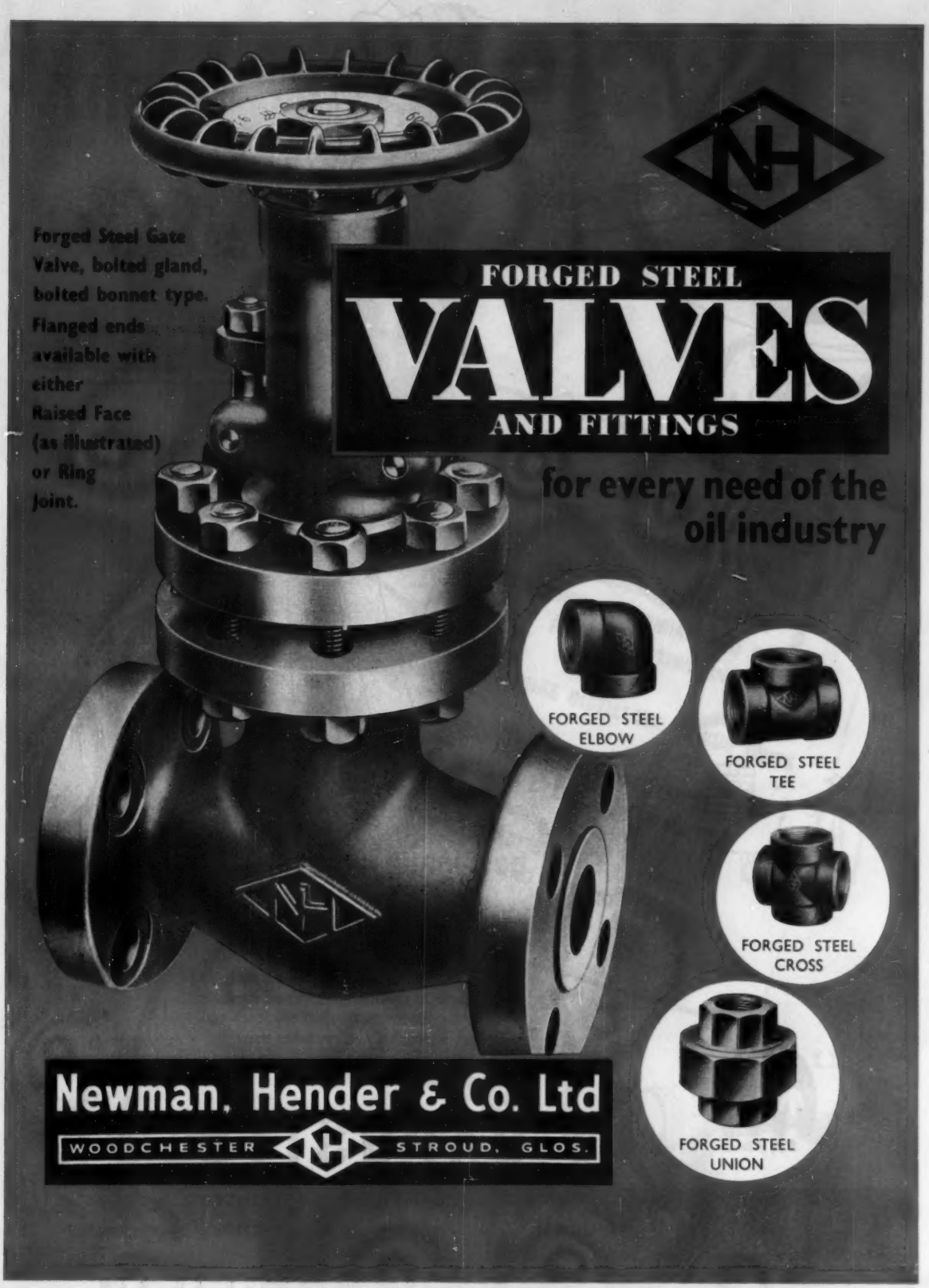


25-RANGE
Used where NORMAL clearance is present, and no serious problem of over-size hole is anticipated.



SUPER-RANGE
"H-50" Used where GREATER-THAN-NORMAL clearance is present and where extensive hole irregularities, or even cavities, are anticipated.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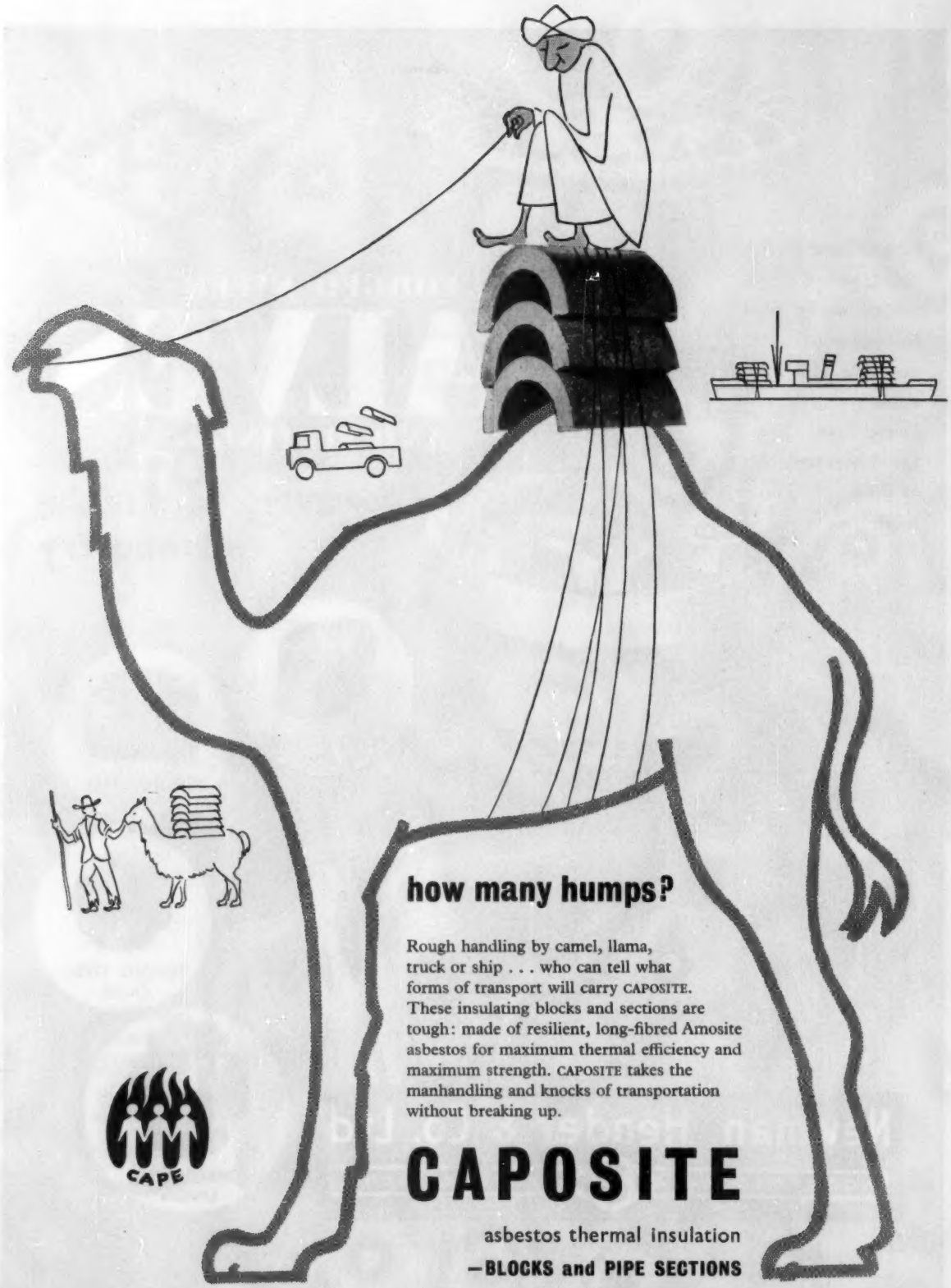

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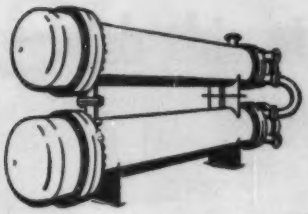

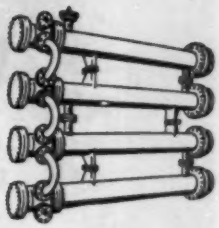

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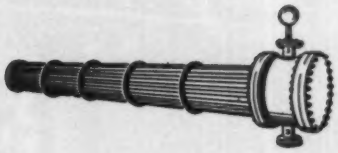

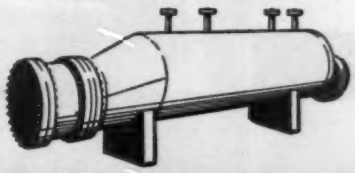

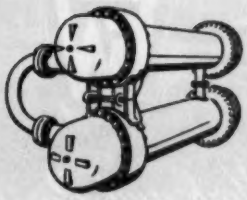

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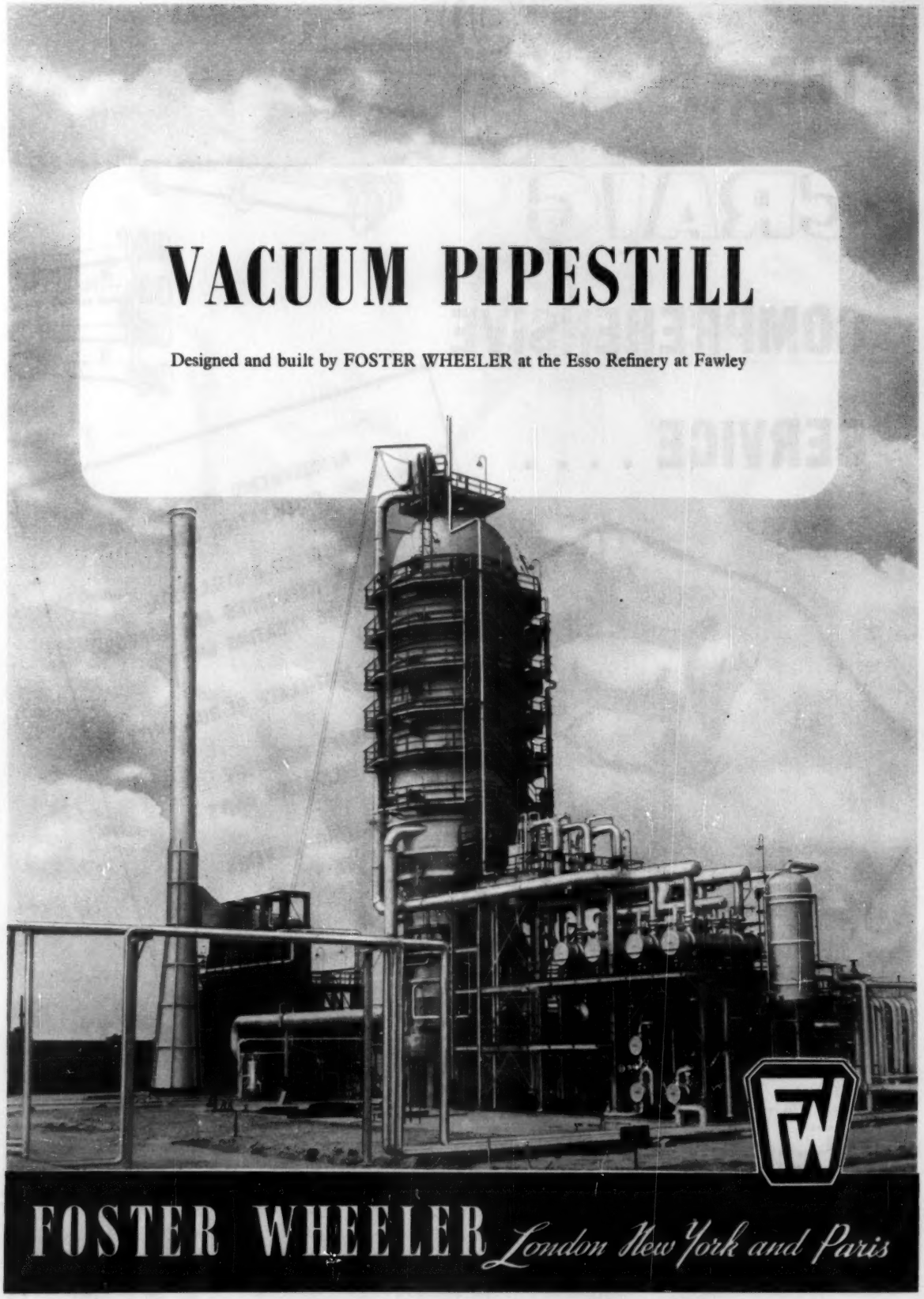
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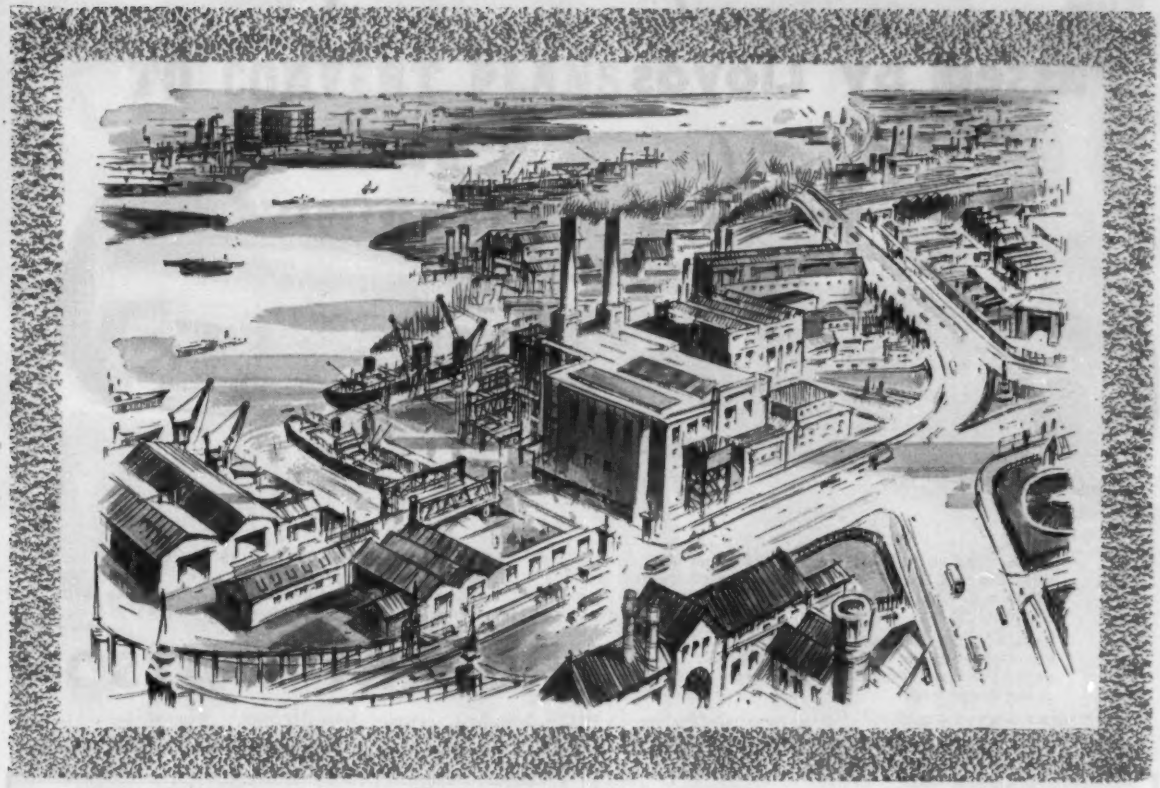

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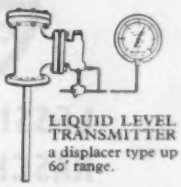

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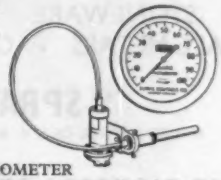

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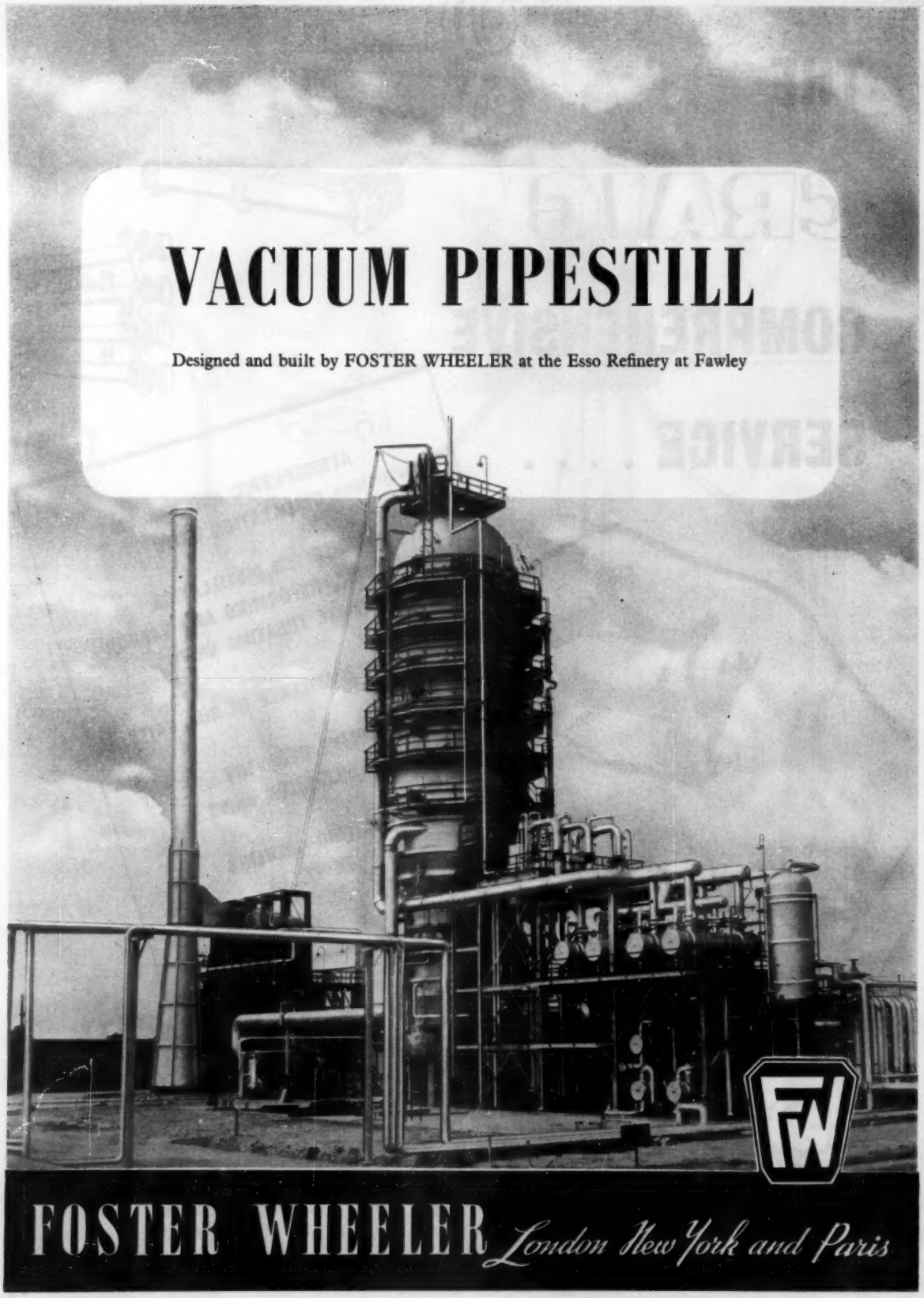
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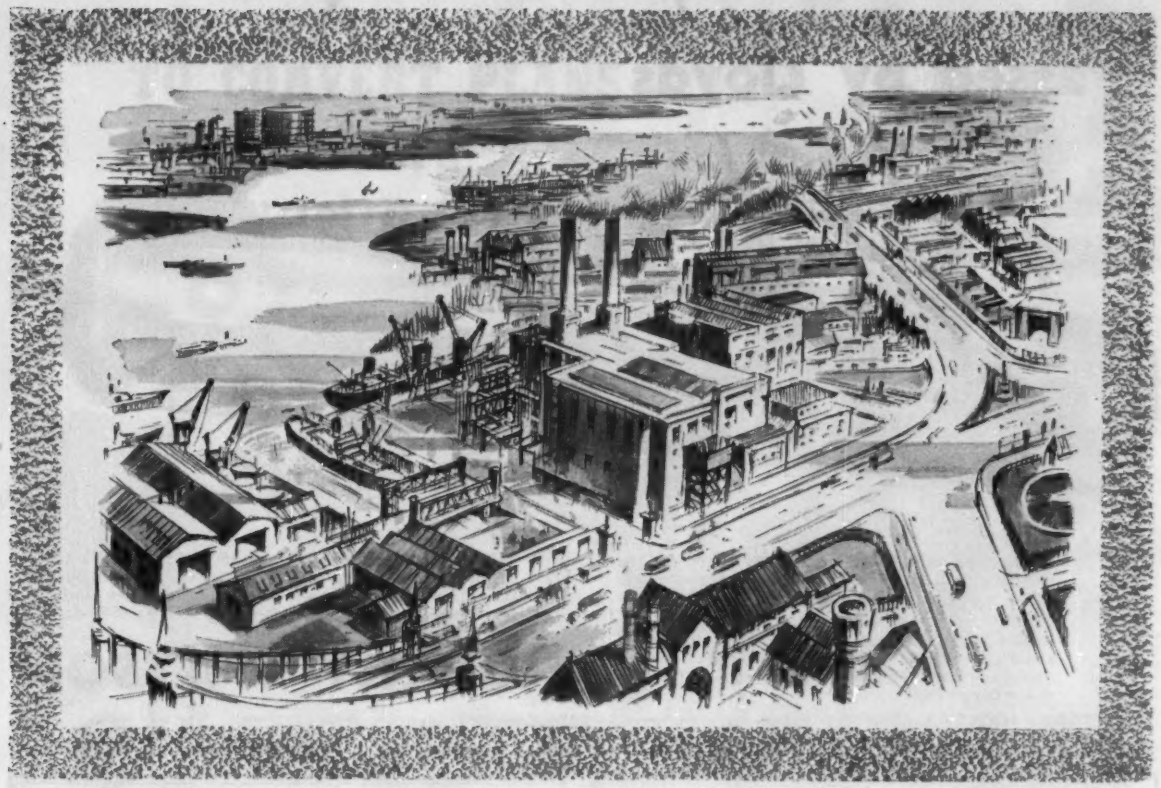

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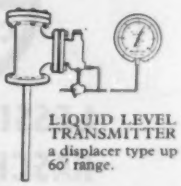

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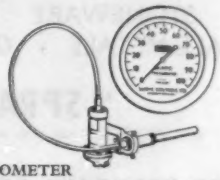

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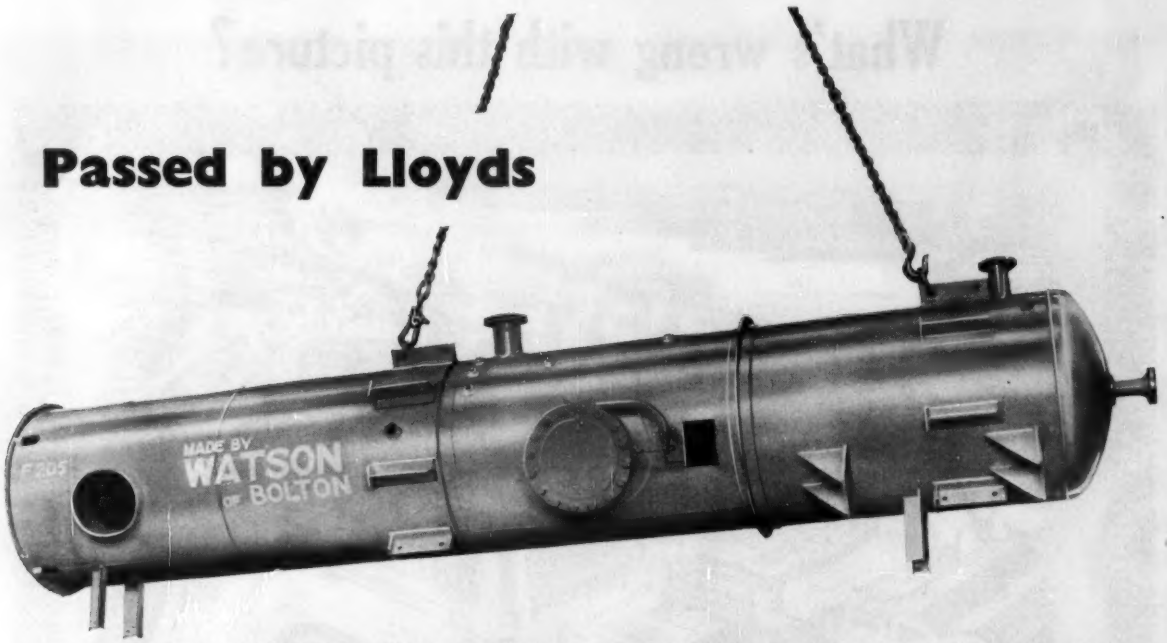

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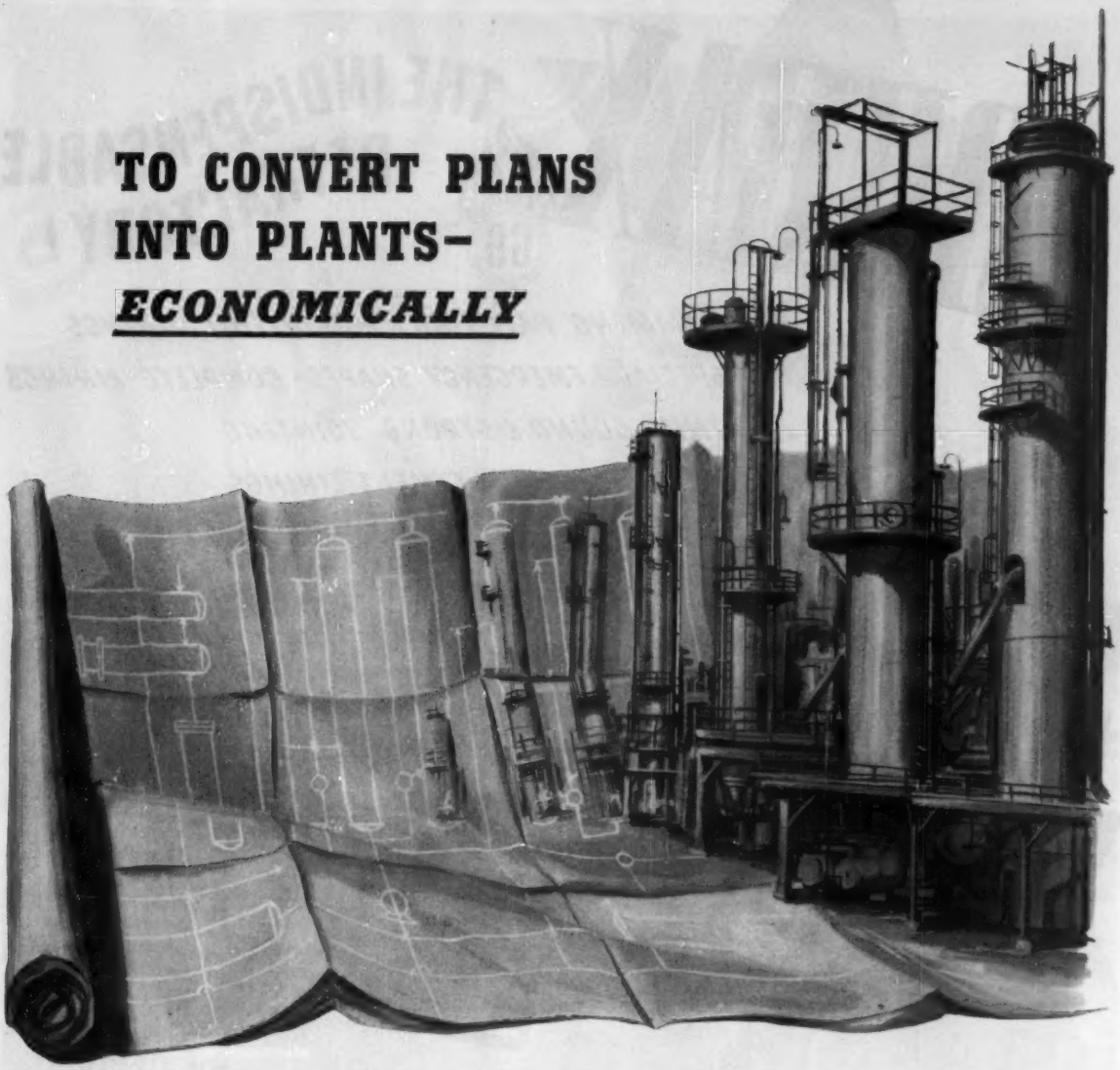

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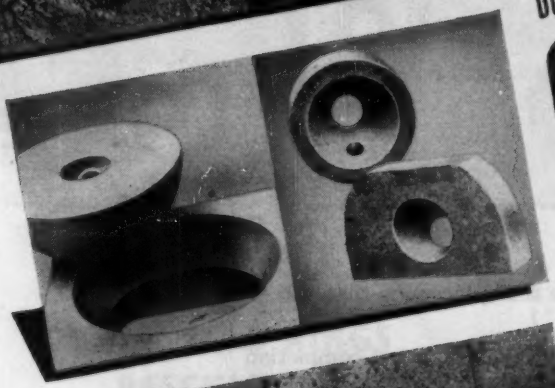

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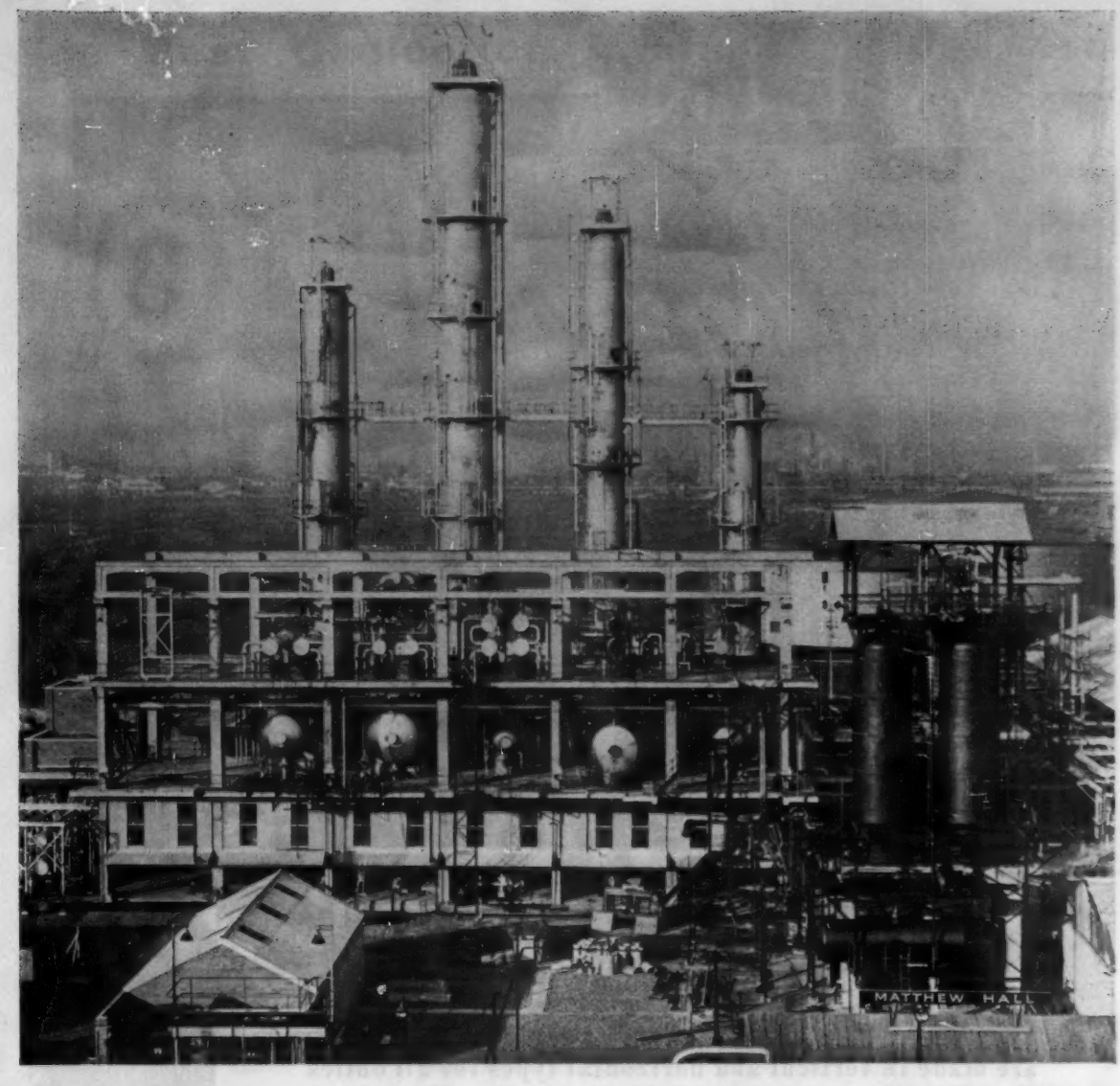

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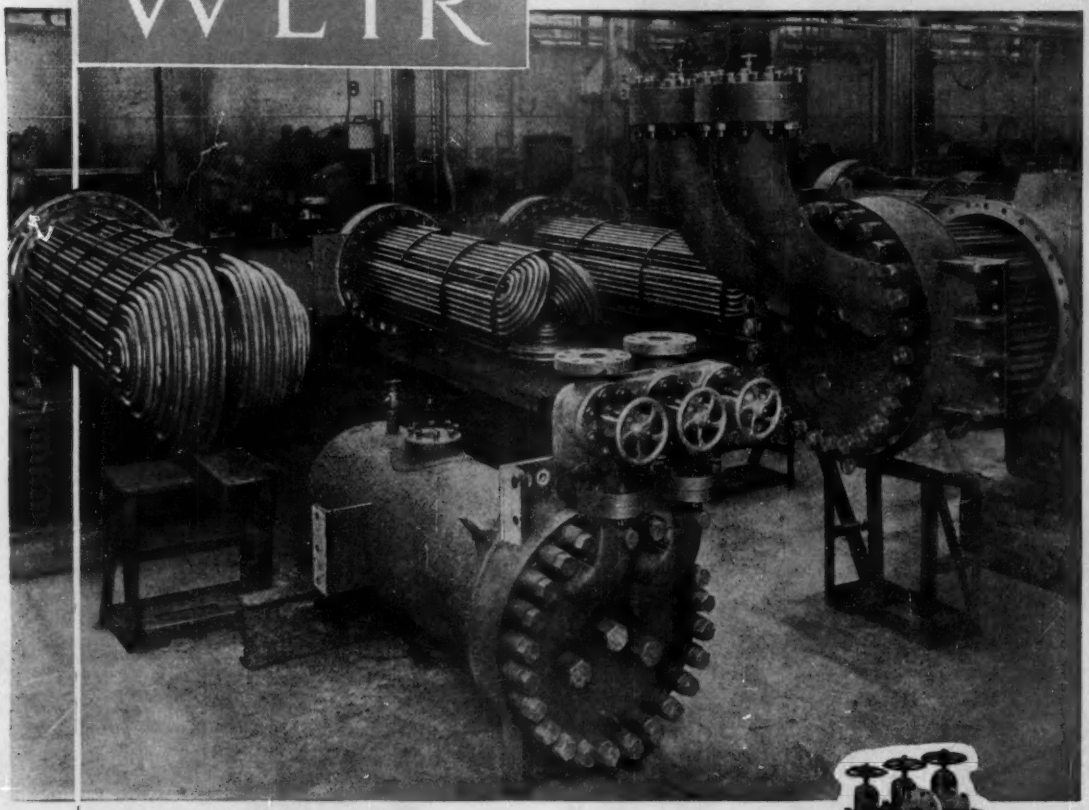

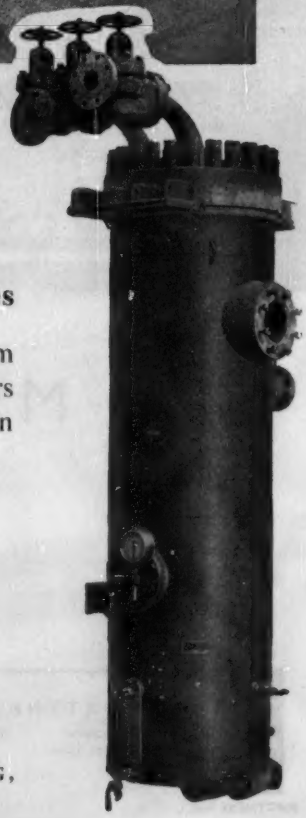

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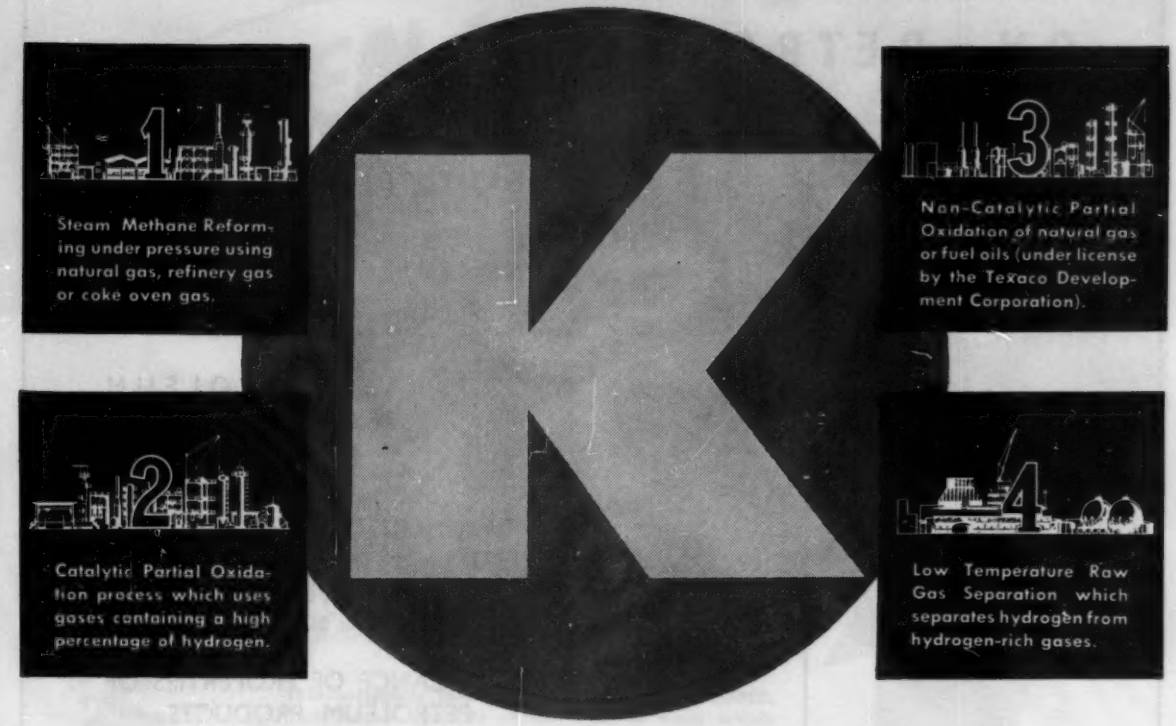
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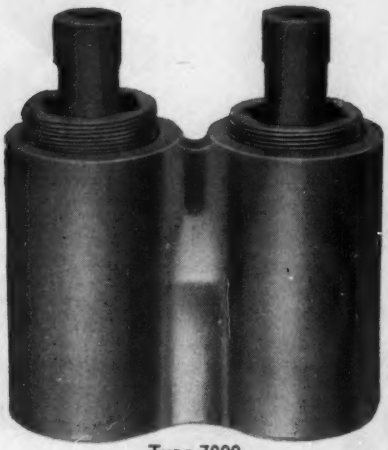

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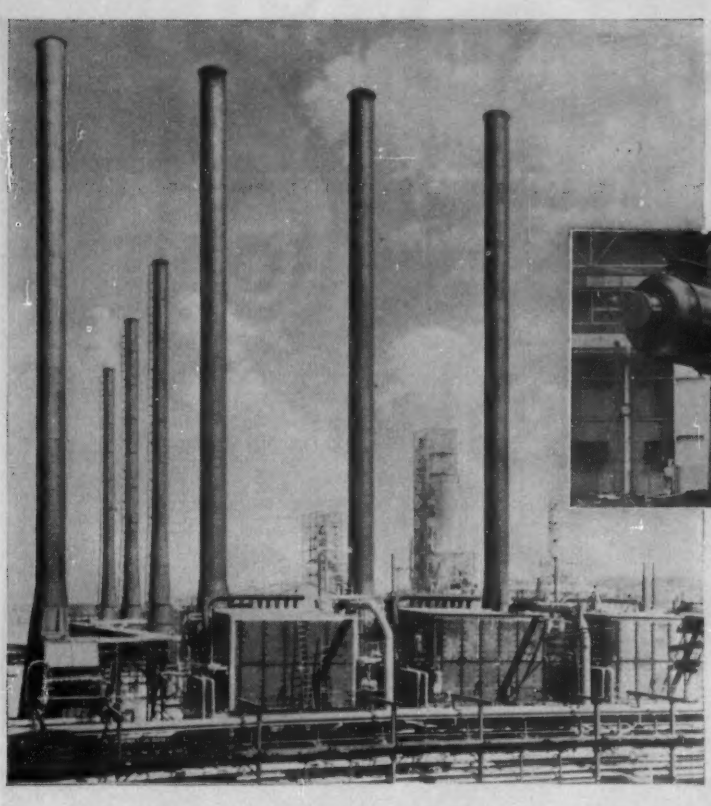
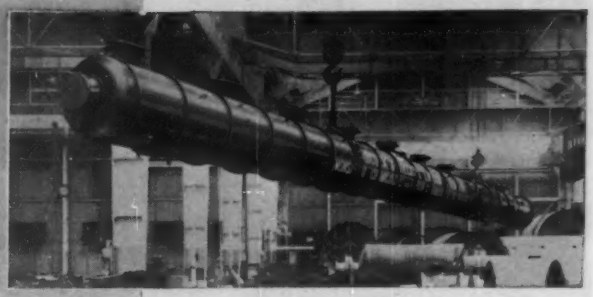


(left)
Babcock oil-fired boiler plant at Burmah-Shell refinery, Trombay, India.



(above)
Babcock fusion-welded treating tower for a Scottish chemical plant.

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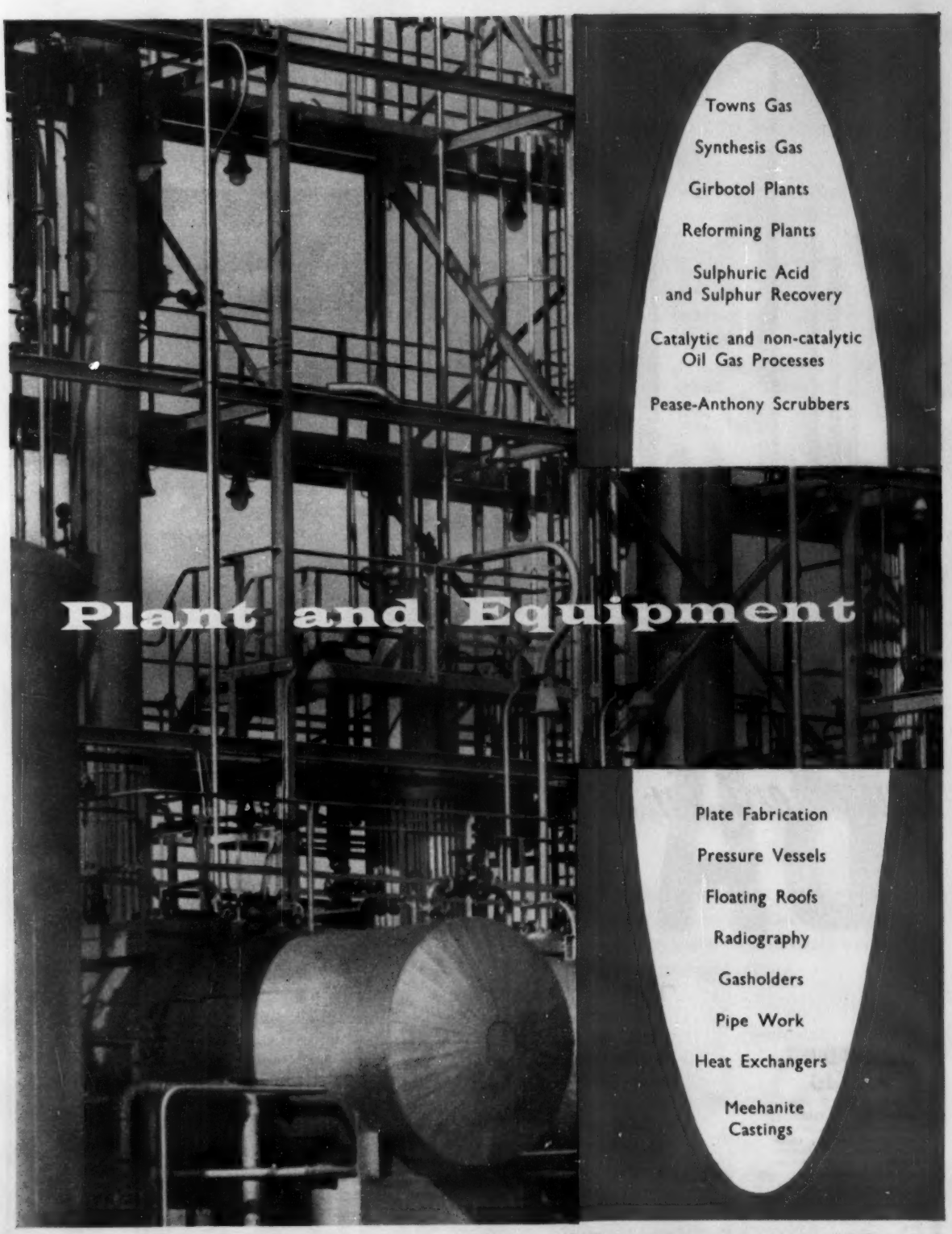

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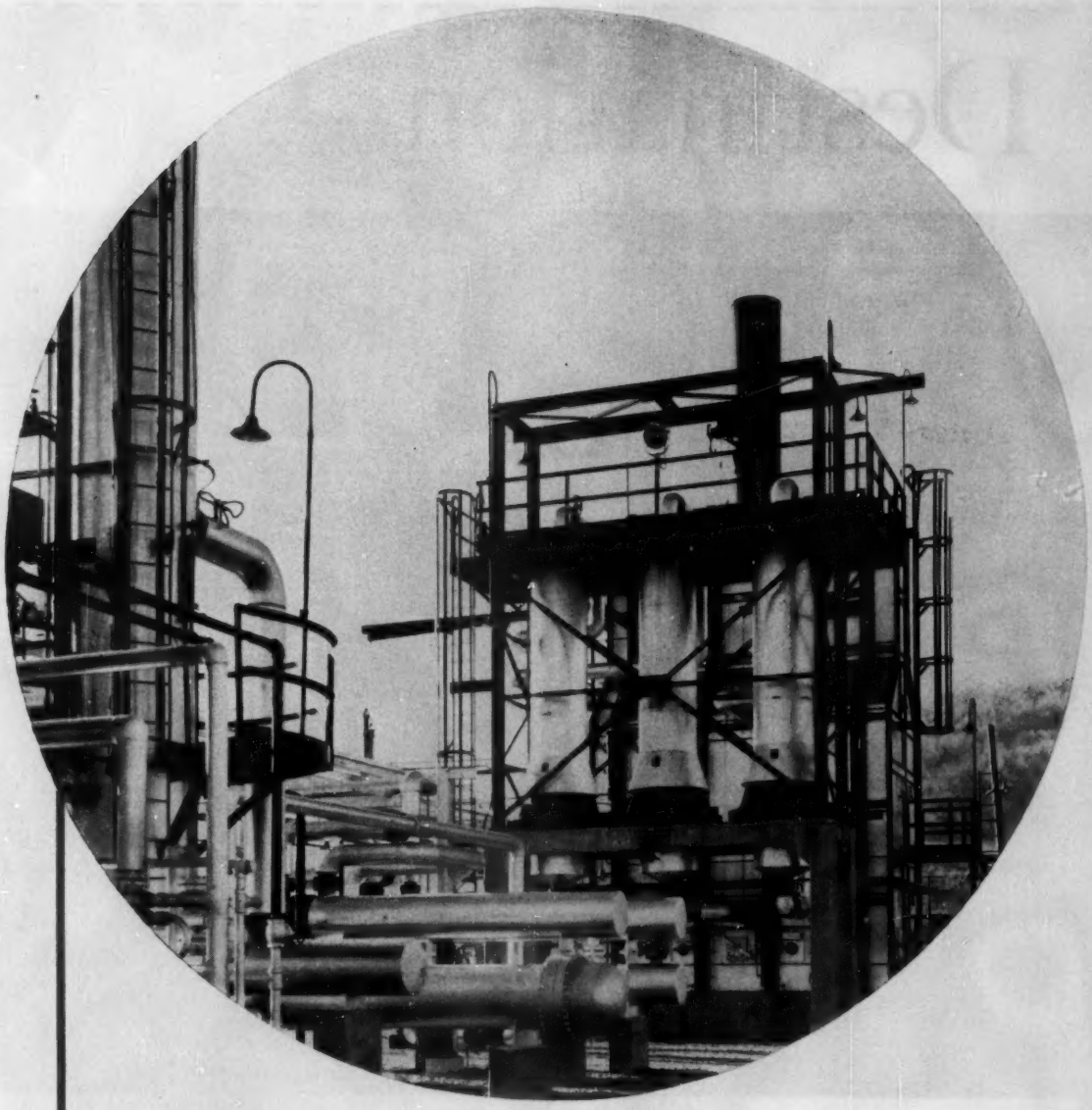

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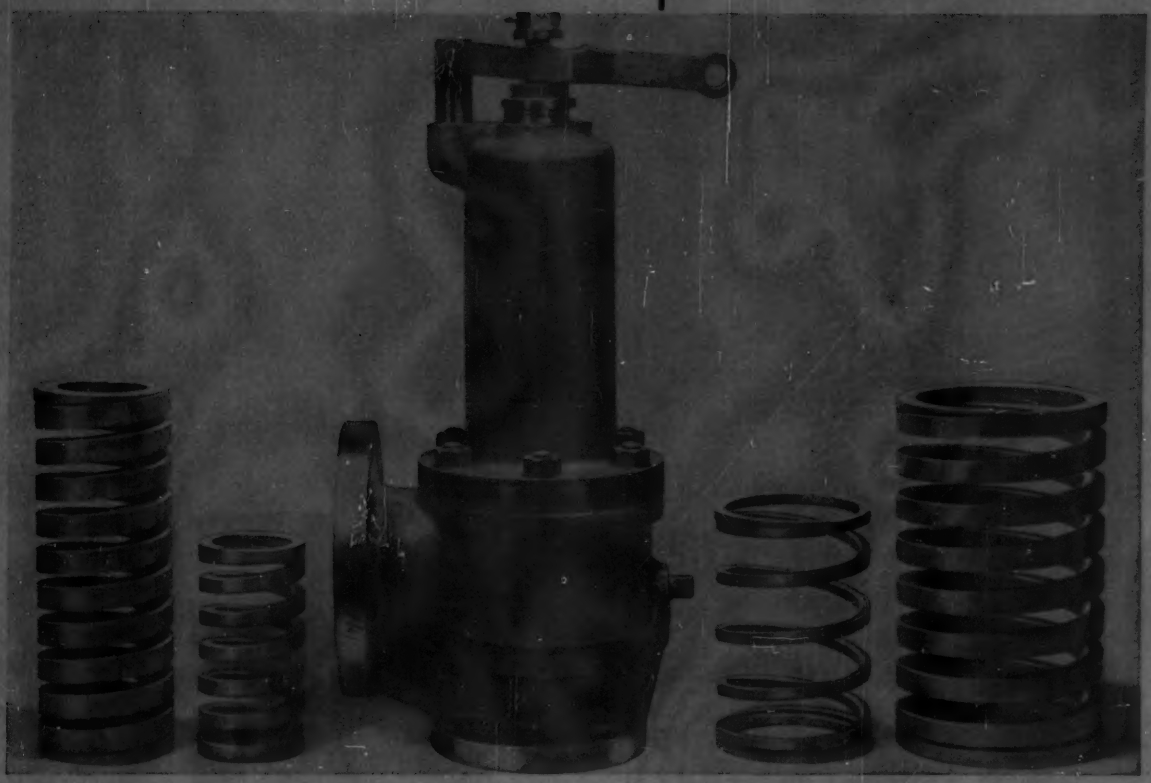

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